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CALGARTH PARK.

DISSERTATIONS .

RELATIVE TO THE

NATURAL HISTORY

OF

ANIMALS AND VEGETABLES.

Translated from the Italian of the

ABBÉ SPALLANZANI,

Royal Professor of Natural History in the University of
PAVIA, Superintendant of the PUBLIC MUSEUM,
and FELLOW of various learned SOCIETIES.

To which are added

TWO LETTERS from Mr. BONNET to the AUTHOR.

And (to each Volume of this TRANSLATION) an APPENDIX,
the first containing a Paper written by Mr. HUNTER, F. R. S.
and the Experiments of Dr. STEVENS on Digestion; the second
a Translation of a Memoir of Mr. DEMOURS, and Mr. DEBRAW's
Paper on the Fecundation of Bees.

V O L. I.

L O N D O N :

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HISTORICAL
MEDICAL



TO THE

BARON de SPARGES and FAIENTZ,

KNIGHT of the ORDER of ST. STEPHEN,

AND

COUNSELLOR to his IMPERIAL and ROYAL
MAJESTY for the ITALIAN DEPARTMENT,

S I R,

YOUR Protection confers Honour
on the Sciences, while your Cul-
tivation extends them: You employ
the Power entrusted to you by his Im-
perial Majesty, in advancing the Hap-
piness of his People, by promoting
the Progress of Letters, and diffusing
A 2 their

DEDICATION.

their kindly Influence. Your Attention unfolds the Abilities of Men, and creates a Multitude of useful Citizens. Your beneficent Activity affords a thousand Resources to all those who devote their Talents to scientific Pursuits; and with this View in particular, you enrich the Royal Cabinet of Natural History at Pavia, by procuring from various Parts numberless Specimens equally rare and valuable.

Yes, Sir, it is You, in whom Europe beholds with Admiration the interesting Spectacle of a Minister, who conceives that he is faithfully discharging his Duty to his Prince, while he enlightens at once the Country and Age in which he lives; and whose Relaxations from Toil, consist in seeking in new Acquisitions of Knowledge, the Means of increasing the Number of happy Individuals.

Allow me therefore, Sir, to solicit your Indulgence for the Work which I presume to offer, as a small Token of
my

DEDICATION.

my lively Gratitude for the many Favours which you have been incessantly heaping upon me. That Sentiment will be ever deeply engraven on my Heart, together with the profound Respect with which I have the Honour to be,

S I R,

Your most humble

and most obedient Servant

LAZARUS SPALLANZANI,

T H E

TRANSLATOR'S PREFACE.

A LONG established custom seems to have conferred upon translators, the right of annexing every epithet of praise, which language affords, to the name of their author. But of this privilege, however valuable, it is not my design now to take advantage. The celebrity of the favourite friend of a Charles Bonnet, and of the philosopher, to whom the dying hand of Haller consigned the defence of Truth and Nature, would not receive much addition from the encomiums of a writer, who chooses to conceal his name. Yet as some uncommon experiments related in the following volumes, may draw the attention of persons but slightly acquainted with Natural History, I think it my duty to endeavour by some means to engage their confidence, lest that which constitutes one of the chief excellencies of the work should,

by creating scepticism in the reader, prove a disadvantage to it. Nothing appears more likely to shew what degree of confidence may be reposed in the philosopher, whom I introduce, than an enumeration of the services which he has already rendered to this useful and amusing branch of knowledge. And I undertake this task the more willingly, since I have observed that they are not very distinctly known, even to some who are employed in this country in pursuits of the same nature.

To sever the different parts of animals, is one of the most effectual means of destroying them. If therefore I should assert that this treatment will multiply some animals, and do no injury to others, the proposition will probably appear, in the eyes of those to whom this Preface is principally addressed, to have all the extravagance of fable: Nothing, however, is more certain. About the middle of the present century Reaumur, the great French philosopher, discovered that some crustaceous animals have the power of reproducing the last joints of their legs. Soon afterwards, the regard of the world was more powerfully attracted towards this topic of Natural History, by Trembley and Bonnet. From their observations it appears, that

that polypes, and several species of worms, are capable of forming anew those parts, of which they have been deprived by design or accident. Some, however, of these animals are constructed with so much simplicity, and they all occupy so low a place in the scale of existence, that Physiologists were not discouraged from attempting explications of phenomena so unexpected and so repugnant to received opinions. Their gelatinous consistence, their ductility, their uniformity of organization, afforded a specious foundation for the theory of animal reproductions (*a*). But the history of every science shews, that at the dawn of new discoveries, the light is too feeble to guide Reason in her progress towards Truth. A short time evinced, that other principles are necessary to the explanation of this wonderful property. In 1768, Spallanzani published his famous Prospectus, in which he announced, that decapitated snails not only survive this dangerous operation, but regenerate the part which they have lost, and that the water-newt reproduces its legs and tail. He moreover asserts, that the same prerogative belongs to frogs and toads in the form of tadpoles. Ever since the date of the Prospectus, the discovery concerning snails has

(*a*) See Contemplation de la Nature, Partie 9.

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been a subject of controversy. The petulance of Voltaire, prompted him to level the shafts of ridicule against the Italian Naturalist. But he had more formidable opponents. The celebrated meteorologist, Abbé Cotte, Adanson, the Historian of Senegal, and Bomare, the compiler of the Dictionary of Natural History, concurred in attacking him. Their objections are founded upon numerous experiments, in all which the snails perished in very different times, indeed, after the separation of the head, but without the least token of reproduction. This contradiction imposed upon the author the necessity of vindicating the reality of his discovery, and he accordingly, in the *Avant Coureur*, a French periodical paper, described the manner in which his experiments had been made. Nor does his testimony stand single and unsupported: it has been confirmed by observers of no vulgar order; by Lavoisier, by Shœffer, by Bonnet and Senebier, so that no weight can now be allowed to the negative results of those who opposed the discovery. Though the work, which thus created a schism among Naturalists before its publication, has been so long delayed, it has not been abandoned. Its appearance may soon be expected. Besides the curious experiments which I have mentioned,

tioned, it will contain many upon the earth-worm. " Spallanzani, says Mr. Bonnet (*a*), has far outstripped his predecessors. He first observed several successive reproductions of the head in the same individual. He has seen pieces cut out of the middle of the body, become complete worms, by producing a new head and a new tail. He has varied the transverse sections, and determined the points at which reproduction no longer takes place. He has ascertained what length every piece must have, in order to regenerate a complete animal. He has traced with care the mode and progress of the regeneration. He has also varied the longitudinal sections. He has attempted, but without success, to form Hydras." It is therefore not unreasonable to infer, that the novelty and variety of the experiments, will amply compensate for the delay.

The next considerable work of Spallanzani, was a series of observations on the circulation of the blood. On a function which has been studied with so much diligence by succeeding physiologists, from Harvey to Haller, modern ingenuity could not be expected to throw much light. The illustrious anatomist,

(*a*) Contemplation de la Nature, p. 249. Ed. nouvelle en 8vo.

whom

whom I have last named, had excluded most of the auxiliary powers, which, according to preceding enquirers, concur with the heart in producing the perpetual motion of the blood. But the Italian Professor, whose observations were made upon cold animals and on the chicken, attributes this effect to the heart alone. He allows nothing to the contraction of the vessels or any other co-operating cause. He shews, that in arteries and veins of equal diameter, the blood moves with equal velocity. I need not inform the reader, that the rapidity of motion was very generally supposed to be much greater in the former than in the latter class of vessels.

Many suppose, that the arterial blood moves more slowly, as it is at a greater distance from the heart. From his numerous observations, Spallanzani concludes, that it moves at least very rapidly in the capillary vessels of that species.

The velocity of the venous blood appeared to increase regularly from the largest to the smallest veins. In the latter the author concludes upon the whole, that it is three times as great as in the former. Though this deduction may seem in some measure to coincide with the doctrines of the mechanical sect of physicians, yet the laws of hydraulics are very

very far from being strictly observed here. For as the sum of the diameters of the small veins many times exceeds the sum of the diameters of the large veins, the difference of velocity ought to be much greater.

The phænomena that offered themselves to the observer's regard in the aorta, were new and curious. In the part contiguous to the heart, the motion of the blood was unequal, and even ceased during the time of diastole. This alternate stagnation became gradually less distinct, as the distance from the heart increased, and towards the more remote region of the aorta totally disappeared. The velocity, however, was not so great towards the end of this principal artery, during the time of the dilatation of the heart, as during its contraction. This remarkable inequality vanishes altogether in the arteries of middle size, such as the pulmonary and the mesenteric, and the motion of the vital fluid becomes quite regular and uniform.

Haller, whose observations on the chicken have so widely illuminated physiology, seems to have been deceived with respect to the primitive colour of the blood. The blood-vessels have, according to him, a tinge of yellow. But the yolk, communicating its hue to them, deceives the eye of the observer.

server. The address of Spallanzani has dissipated the illusion. He detached the vascular membrane from the yolk, and setting it upon a plate of glass, found that the yellow colour disappears, and is succeeded by a pure red.

Of the red particles of the blood he observed, that they do not turn round, according to a common supposition, upon their own axis, and that they have merely a progressive motion along with the other constituent parts of that fluid.

Thus the testimony of Spallanzani may be added to that of others, in order to prove, that the laws to which inanimate matter is subject, cannot be applied to living bodies. It could not, however, be expected, that mechanical physiologists would yield without a contest to this attack upon their theory. Accordingly Prochaska, a physician of Vienna, has attempted to overturn these observations. But his objections will only serve to strengthen the cause of his adversary; and it would be easy, if this were the proper place, to expose their futility. It may perhaps be sufficient to remark, that he has not taken up the microscope, and seen the contrary of what is related by the Italian Professor, but with a temerity that can scarcely be

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be too much reprobated, has opposed reasoning to experiment.

Those diminutive beings that inhabit infusions and the seminal fluid, have been more signalized than larger animals, by the disputes and the theories of philosophers. Since the æra of that discovery, which laid open a world before invisible to human inspection, Naturalists have enquired into their origin and nature with anxious curiosity, but with a degree of success by no means proportional to their labour. The subject was involved in uncertainty and contradiction, when Spallanzani undertook to investigate it; and in this enquiry, in which the union of genius and patience, invention and assiduity is so indispensably necessary, it will not be difficult to shew, that he has succeeded better than any other observer. In the year 1765, he published *Microscopical Observations relative to the Systems of Generation of Messrs. Needham and Buffon*. Continuing to study this curious subject with unremitting attention, he produced ten years afterwards a larger work, in which most of his former ideas appear confirmed by many repetitions, and amplified by important additions. Of this work I am about to gratify the reader, by a brief analysis.

London, 1775. Printed by J. D. Newbery, in Pall-mall. His

His first essays were made with a view to ascertain the effects of heat. He boiled, roasted, and even calcined several substances; but these violent processes, when conducted in open vessels, did not prevent the appearance of the animalcules. Nay, the infusions of the substances that were boiled longest were the most populous for boiling, by hastening their decomposition, contributes to a more excessive multiplication. In vessels hermetically sealed, very different phenomena occur. The larger kinds (for these animalcules differ very widely in size) do not appear if the infusion boils a single second; and so deleterious is the power of heat under these circumstances, that they are not evolved in a warmer temperature than that which is marked by the 92 or 93° of Fahrenheit's thermometer. The more diminutive species sustain, in close vessels, a boiling heat for half an hour, but not for three quarters. The author, desirous of illustrating his subject by resemblance and contrast, was very naturally led to enquire how far other animals and vegetables are capable of resisting the effects of heat. He found in the first place, that the eggs of certain animals possess this privilege in a more eminent degree than the animals themselves. Thus tadpoles and frogs perish at 110°, 111°, whereas no degree of heat short of 133 destroys

stroy their eggs (*a*). Silk worms, and the erucæ of the butterfly of the mountain ash, are unable to sustain the degree of heat expressed by 108° ; but the eggs of both these species are not rendered unprolific by any degree lower than 133° , and many bear an hotter temperature with impunity. Large flies die at $99^{\circ} \frac{1}{2}$, their nymphs 110° , 111° , their maggots at 108° , and the eggs are spoiled only at 140° .

The same law extends to plants, and the seeds from which they spring. The seeds of the bean, the French bean and trefoil, sustain unhurt the heat of boiling water, whereas the plants themselves are destroyed by the temperature designed by the 167^{th} of Fahrenheit's thermometer:

The seeds of plants are more capable of resisting the action of fire than the eggs of animals. Of the former, all on which the experiments of Spallanzani were made, resisted a dry heat of 167° , and many of 212° . But not a single egg was hatched after it had sustained the 145^{th} of Fahrenheit's thermometer.

(*a*) It will be seen in the second Volume, that these bodies are not eggs; but this circumstance does not in any way affect the result of the experiment.

I say a *dry heat*, because it appears that fire, in combination with water, acts with greater efficacy.

Thus, if we except the larger animalcules of infusions, the analogy between the inhabitants of the microscopical and the visible world is complete, and thus every new observation in Natural History, affords a fresh example of the beneficent care of Providence. By what means HE, whose wisdom has ordained, that the generations of created beings should follow each other in a regular succession, has secured this privilege to the rudiment of the future animal, who does not wish to be informed?—But its extreme minuteness keeps it so remote from every sense of man, that the imagination is left to form conjectures unassisted and alone. The difference, which is great, and consists of many degrees of heat, forbids us to suppose that the covers, under which the germ is lodged, can be the cause of so remarkable an effect. Nor can its smallness be admitted as a satisfactory reason, since we cannot but suppose the particles of heat to be still smaller. Is the phenomenon owing to the dimness of the spark of life? Analogy teaches, that this spark is less easily extinguished in those cases where it is less vivid. Thus animals in a
state

state of torpor do not so soon die after violent operations, such as decapitation, as when the vital energy is strong; and insects plunged in water, do not drown in winter so soon as in summer. The hypothesis is not without plausibility, and the Physiologist will suppose, without much reluctance, that the principle of life is less intense before fecundation.

The Author next passes, by an easy transition, to an examination of the effects produced by cold. He found that the germs of these minute beings resist a very intense cold. The animalcules themselves of a smaller species are not destroyed, as long as the water retains its fluidity. And even in this case, it is the congelation of the water, and not the absence of heat which proves fatal; for in water, in which the thermometer sunk 16° below the point of congelation, but which, by being kept at rest, was prevented from being frozen, they survived, though their movements became indeed less brisk. It deserves to be remarked, that the species which sustained this rigorous temperature, agreed perfectly in appearance with those which bore the greatest heat.

In the prosecution of his experiments, the author found, that the animalcules of infu-

sions are destroyed by the odour of camphor and turpentine, by the smoke of tobacco and the vapour of sulphur, by urine and other saline substances. The electrical fluid also kills them instantaneously, and they agree with other animals in requiring the influence of the air for the support of life. Some species, however, are entitled to the notice of the Naturalist, on account of the length of time during which they live in void space. If some perish in a day or two, there are not wanting others which can support the absence of air for a month. Nor do they immediately discontinue the exercise of their functions, or abate of the quickness of their motions.

Since experiment has succeeded to conjecture, and observation grown more accurate, men have diligently endeavoured to trace, by deductions from facts, the origin of animated beings. Among the systems which have been invented, several have been built upon the inhabitants of infusions, and the seminal fluid. Writers, finding nothing in the production of large animals to favour their paradoxical opinions, have sought for refuge in the obscurity of minuteness. Needham asserts, that certain filaments, observable in infusions, are animated by an internal expansive

five spirit, and that after swelling and performing certain irregular progressive motions, they divide into small pieces, which being more and more exalted by the expansive force that is continually purifying them, are at last converted into microscopical animalcules. This wonderful transmutation of vegetables into animals, Spallanzani has diligently, but in vain, sought. He has seen animalcules appear before filaments, he has seen animalcules in infusions which never produced any filaments, or in other words, any mould, and he has seen filaments where there were no animalcules. Besides these decisive observations, he has discovered the source of Needham's errors. Among the animalcules of infusions, there is a species distinguished by a long filament at the posterior part of the body, by means of which it fixes itself to the small vegetable productions; this species is multiplied by the division of one individual into several others. It must likewise be remarked, that among microscopical vegetables, there are funguses in miniature. They consist of a large head supported by a slender stalk. The mistake, therefore, which gave rise to all these wild ideas concerning generation, was sufficiently natural. It consisted only in confounding two productions,

a 3

productions, which, though they are so essentially different, do not ill agree in outward appearance.

The various ways in which the several species of animalcules are multiplied, are among the most curious phænomena which the microscope discovers. Some divide into equal parts; the reader will have a clear conception of this mode of division, if he will form to himself the idea of an inflated bladder, with a ligature thrown round the middle of it. This will represent the first appearance of division. The transverse furrow gradually becomes deeper and deeper, and at last the two individuals are connected only by a point. Then an unexpected scene passes before the eyes of the spectator. The other animalcules, though they at all times besides carefully avoid impinging against each other, now rush, by one consent, upon the place of division, and the assistance they thus afford, together with the efforts of the united individuals, at last effects a separation. This appearance of social instinct, in beings placed at such a distance from those which we are most accustomed to contemplate, will perhaps seem apocryphal to those to whom I am writing, so much has it the air of a tale borrowed from a description of fairy-land. It is

is therefore proper to inform them, that it stands amply confirmed by the testimony of other naturalists of veracity equally unsuspected. Nor are examples wanting of a natural division among animals more easily accessible to the senses.

Some species are multiplied by a longitudinal division, which sometimes begins at the fore part of the body, and sometimes at the hind part. Thus far the division is equal; but there are also examples of inequality, a small piece only being detached from some kinds.

A superficial structure between two globules, may in some instances be perceived. The observer supposes that this structure will, as before, become gradually deeper, till it terminates in a point, but is surprized by an instantaneous separation.

Groups consisting of four or five globules, are often seen to float for some time in an infusion, and afterwards to separate into bodies, which are at first round, but in time are split into lobes, that separate in the same manner.

There is another mode of division yet more extraordinary. Let the reader imagine a spherical body formed of concentrical strata, of which each consists of a number of animalcules. Those which compose the super-

ficial stratum, part from the others, and swim about the liquor. The next in succession do the same, and so on till the sphere is decomposed.

These are not, as may very naturally be imagined, a cluster of animalcules that unite and separate at will. The author's observations sufficiently prove the contrary, for when he kept one apart from the rest, he found that it grew in three or four days to a perfect resemblance of the sphere from which it sprang.

Of these modes of multiplication, it may in general be observed, that they are forwarded by heat, and retarded by cold. The division which in winter requires several hours, is in summer effected in a quarter of an hour.

The inhabitants of infusions afford oviparous as well as viviparous individuals. In all multiplication is exceedingly rapid. A single individual soon produces an offspring too numerous to be counted; and the quantity in a short time surpasses not only belief but imagination. "Their number, says the author, exceeds that of the integrant parts of the fluid. All putrefying substances, and water of every kind, contain prodigious multitudes. Every drop is peopled with myriads.

Let

Let the reader now reflect upon the quantity of running, stagnating, and salt water, and then let him try to conceive the number of these animals."

If unsatisfied curiosity should seek to penetrate yet a step further into the origin of animalcules, the experiments of Spallanzani will supply the means of gratification. As the phials in which infusions are contained are more or less closely confined, a smaller or larger number of animalcules appears in them. Hence it is probable, that the atmosphere is the mighty magazine in which Nature has repositied her immense stores of the germs of these diminutive beings.

The manner and variety of their movements deserve likewise to be noticed. Some, like eels, proceed by the contortion of their bodies; points and filaments growing out of the body are the means by which others are enabled to advance. Some move slowly, others with a swift pace. Several species are distinguished from all other productions of creative wisdom, by being unacquainted with repose. In one species certain filaments placed at the posterior part of the body, by being alternately bent, and suddenly made streight again, project the animal to a great distance, as an arrow is shot from a bow. Some continually

tinually revolve like tops upon their own axis, but this rotatory motion is attended with a progressive one.

The means by which they sustain life, are not the least engaging part of their history. Some excite an eddy by moving rapidly the filaments that are placed round the opening of the mouth, and thus draw towards them the particles of food that float in the liquor. Others, with all the ferociousness of rapacious animals, pursue their weaker fellows. In these, which may be termed animalcules of prey, it is pleasant to behold the repose that follows satiety, and the exertions that are excited by hunger.

This sketch shews, with sufficient evidence, the falsehood of the principles upon which Needham has constructed his system of generation. The animalcules of infusions neither spring from vegetables, nor can they be considered as other than real animals.

Having atchieved the overthrow of Needham, Spallanzani proceeds to encounter a more formidable antagonist. Buffon, whose skill in composition has rescued the French language from the imputation of feebleness and want of nerves, has founded a theory of generation on the corpuscles, which the microscope discovers in the seminal fluid. This
fluid

fluid he affirms to be at first full of ramified filaments, which, after a certain interval, burst and emit a number of small oval bodies, attached to the filaments by a long thread. After some oscillations they liberate themselves and move about, though but slowly, on account of the incumbrance of the long thread or tail which they are obliged to drag after them. They afterwards lose this thread, and then their motions become quicker. Some seem to change their shape and size, and then also the velocity of their movements grows gradually greater. The observer frequently sees the oval changed into the globular figure, and one corpuscle divided into two. Such are the observations from which the French naturalist concludes that these corpuscles are not real animals. The loss of their tail, the change of their figure, their formation under the eye of the observer, their division and gradual diminution of bulk, are circumstances which, in his opinion, concur to exclude them from the animal kingdom. But as they have an innate power of motion, and shew signs of vitality, he on the other hand separates them from inanimate matter, and creates a particular order of existences, under the title of *organic molecules*. To these molecules,

cules cast in certain internal moulds, he attributes the formation of all animated beings.

But this theory can no longer subsist without other proofs, for Spallanzani has taken from it all the support it received from the inhabitants of the seminal liquor. His observations on this subject, bear so close a resemblance with those on the animalcules of infusions, that I shall forbear entering into a particular account of them. He has brought irrefragable proofs that they do not spring from filaments, that they never lose their tails, and that their size does not diminish or their activity increase in a regular progression. He has shewn, by comparing their properties with the properties of other animals, that they have an indisputable right to be ranked among the productions of the animal kingdom. Buffon describes them as surviving several days after they have been exposed to the influence of the air, but Spallanzani always found that they died in a few hours.

Concerning narrations thus contradictory, the determination of naturalists would be different, and each would become a partizan of the French or Italian philosopher, according to his opinion of the veracity and skill in observation of the one or the other,
if

if the latter had not dissipated every doubt, by exposing the origin of the other's mistakes.

When the seed has been taken from its receptacles, and the spermatic worms have lived the short space afterwards allotted to them, this fluid, like every other animal substance, acquires new properties, and loses those which it before had. A new race of inhabitants succeeds sooner or later with a shape, structure, and movements, totally different from those of the first, but agreeing with the form, which, according to Buffon, the organic moleculæ assume. Agreeably too with his description, the several successions become gradually smaller, till at last the eye of the observer loses them in indiscernable littleness. These occupiers of corrupted semen, are in no respect different from the animalcules of infusions, but from the spermatic worms they are separated by every mark which can enter into the description of the naturalist. The structure, the shape, the manner and velocity of motion, the place of residence, the effects of air, afford so many discriminative signs.

Before these experiments, the theory of Buffon vanishes as an enchanted castle at the approach of the destined knight. But it is pleasing to observe, that while they shew the falsehood

falsehood of his system, and the inaccuracy of his observations, they are favourable to his veracity. In his descriptions truth is so entangled with error, as to shew that his judgment, and not his intention, was in fault. A suspicion, not perhaps destitute of plausibility, has been suggested, that he had seen the organic molecules with no other eyes than those of the imagination. I therefore experience no common satisfaction, while I bear this testimony to the character of that sublime and original genius, who, by painting the works of God in colours worthy their variety, their beauty and their grandeur, has rendered the History of Nature more alluring than the fictions of poetry.

If I may be allowed to introduce an incidental remark, I would turn the attention of the reader to the difficulty, under which the most ardent imagination must labour, when it attempts to conceive the production of organized bodies, without the aid of some pre-existing cause, not essentially unlike germs. In whatever proportion the several forms of matter are combined, and in whatever manner the powers which Chemistry chiefly discovers that they possess, are supposed to exert themselves, yet when a mind, neither influenced by the love of paradoxical opinions,

opinions, nor actuated by the desire of contradiction, tries to imagine how an animal can be formed from such materials arranged by such causes, it turns aside from the speculation in hopeless despondency. When Buffon substitutes his organic molecules and internal moulds, he unluckily proposes a theory encumbered with all the difficulties attending the hypothesis of germs, but destitute of its advantages.

In Spallanzani's experiments on the effects of confined air, his usual accuracy may be recognized; but that originality of conception, which distinguishes his other works, is not so striking in this essay. The public attention, at least, has been so much turned towards elastic fluids, so much has been written concerning the effects of vitiated common air, and the several species of gas upon animal and vegetable life, that an analysis of these experiments would be a repetition of what is to be found in a vast variety of publications. I will only observe, that he attributes the death of animals, kept in unchanged air, to exhalations acting on the nervous system. If he is acquainted with the writings of Priestly, he will probably have found reason to give his hypothesis a modification somewhat different.

Certain

Certain animals, which lose every property and appearance that distinguish them from inorganic matter, and recover the exercise of their functions, have drawn the regard of Spallanzani. To the information afforded by Leuwenhoeck, concerning the rotifer, he has added many curious particulars. He has discovered some new species endowed with this remarkable property. He has shewn, that though they may be resuscitated after a long cessation of the powers of life, and a certain number of times, yet these qualities have their limits; nor does immortality belong to these extraordinary productions of Nature, any more than to other created beings. He has described the phenomena, the loss and recovery of vitality, with minuteness and precision.

The chief aim of the experiments on moulds, is to trace the origin of that minute vegetable. It had been rendered probable by Micheli, the famous botanist of Florence, that mould is propagated by granules, shed in great abundance from the bursting heads of the plant. Against this opinion, Mofcati advanced some very specious objections; and upon the whole concluded, that mould is the produce of spontaneous generation. But Spallanzani has determined the contro-
versify

verfy in favour of the Florentine naturalift, by many well-imagined and well-executed experiments; fo that both microfcopical animals and vegetables agree in their origin with thofe that have been conſtructed upon a larger model.

In the works which have ſupplied me with the foregoing obſervations, there are ſeveral incidental diſquiſitions, which will amply reward the attention of the reader. But I muſt omit them, leſt this preface ſhould be extended beyond all bounds. There is, however, one digreſſion ſo intereſting in itſelf, and at the ſame time ſo characteristic of the genius of the author, that I am tempted to produce it here.

Among the phænomena recorded in Natural Hiſtory, I recollect none more extraordinary, than that ſtate of torpor into which many animals fall at the ſetting in of the cold ſeaſon. By what means this great change is produced, whether the fluids are rendered unfit for circulation, whether the nerves are deprived of their influence, or whether the muſcular fibre is no longer ſuſceptible of that influence, philoſophers have not, till very lately, thought of enquiring. The great claffical writer of France, the illuſtrious Buffon, firſt inveſtigated this curious topic

of comparative physiology. He considers the temporary cessation of the powers of life, as arising from the refrigeration of the blood. From several thermometrical observations he concludes, that the temperature of these animals is equal to that of the atmosphere, and regulated by it. But the Italian Professor, having repeated these observations with his habitual accuracy, found, that the subjects of Buffon's experiments are not to be numbered among animals of cold blood. In their mouths he has seen the liquor of the thermometer rise to the 101st degree, at a time when in the open air it stood at 35° or 36°. But the genius of Spallanzani did not rest here. By an experiment which alone would have raised its inventor to an high rank among Naturalists, he at once completed the overthrow of Buffon's theory, and ascertained, at least with great appearance of probability, the cause of the phenomenon. The tenacity of life by which frogs, toads, and other amphibious animals are distinguished, is sufficiently known. The heart of these animals may be cut out, not only without immediately destroying them, but without retarding the exercise of their functions, or impairing the vivacity of their movements. Taking advantage of this singular property, the author

evacuated the vessels of several frogs, newts, and toads. He then buried them in snow; the expected effect followed, and they soon became torpid. Being exposed in this state to a proper temperature, the suspension of life was succeeded by a complete recovery of the use of their vital organs: insomuch that there did not appear to be any difference between those which had sustained the loss of their fluids, and those which were subjected to the experiment unhurt and entire. The cause, therefore, of this lethargy, is not connected with any condition of the blood; it seems to depend on the privation of the irritability of the muscular fibre: Spallanzani at least observed, that no stimulants, not even the electrical fluid itself, produced the smallest contraction of the muscles.

I am aware, that this enumeration of the productions of Spallanzani is imperfect; I have purposely omitted a translation of the *Contemplation de la Nature*, and some smaller pieces, such as several articles in the Prospectus of the Siennese Encyclopædia. Notwithstanding the freedom of modern intercourse between different nations, and the advantages of commerce, foreign books are not so regularly imported into this kingdom, but that some original and important work may

have escaped my enquiries. But the rude sketch which I have exhibited, will, I hope, fulfil my wishes, and induce those who have no better means of information concerning the worth of this distinguished philosopher, to hear him with respect and attention.

Of the following work, it is not necessary to attempt an analysis. Its importance cannot fail to strike the most careless observer. In the first volume, we have a complete history of one of the chief functions of the body. The second dissipates much of the darkness that has brooded for so many ages over the process of generation. When, however, I view these splendid discoveries in their brightest light, when I consider them in their relation to the art, of which the object is the health of mankind, I cannot but own with regret, that they have rather a negative than a positive merit. Like other great advances in physiology, they serve rather to extirpate error, than to afford materials of very high value for the true theory of medicine.

In executing the following translation, my principal aim was to give the precise meaning of the author. My secondary object was, to preserve un sullied the purity of my native language.

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The first of these intentions, I hope, I have fulfilled; and I may, I think, venture to assure the reader, that he may consider the following work as the genuine representation of the original. It may, however, be necessary to apprise those, who shall compare the English with the Italian, that I was compelled, however reluctantly, to take some liberties with the author's phraseology. His desire of being perspicuous, has sometimes led him into diffuseness. There occur too frequently expressions which clog the sentence, without introducing any new idea, or illustrating any proposition already advanced.

This defect did not fail to strike me on the first perusal of the Dissertations. But, persuaded that the translators of books of science and of polite literature, ought to hold very different objects in view, that exactness is the business of the one and elegance of the other, I translated the first dissertation very closely. But when I came to consider my attempt, I immediately perceived, that the ear of no reader would tolerate it; and some judicious friends concurring with me in opinion, I threw it aside, and began the work again, with a determination to prune this useless luxuriance. I had performed more than half my task, before I met with a passage in a judicious author,

thor, which at once confirms my ideas, and justifies the freedoms I have taken. Mr. Bonnet, in a letter to Spallanzani himself, uses the following terms. “ Vous êtes, en general, très-clair & très-méthodique. Je vous exhorterai seulement à vous resserrer un peu plus dans certaines descriptions, où vous employez plus de mots, qu’il n’en est besoin. Evitez encore les pléonasmes & les synonymes : ils n’ajoutent rien à l’idée (a).

That I have attained my other purpose I dare not affirm. When the mind is moulded to the idiom of one language, it cannot immediately receive a new impression without retaining some traces of the former. On this account, it may safely be asserted, that translation, as far as mere expression is concerned, is the most difficult species of composition. When I read what has been printed for some time, I easily perceive, that many phrases might be made neater and smoother, though not perhaps more perspicuous, which I consider as the chief excellence of a work of this kind.

That I might the more effectually recommend my translation, I have added some things, which are not to be found in the original.

(a) Œuvres ed. in 8vo. tom. XI. p. 322.

The

The experiments of Dr. Stevens will doubtless be well received. Such academical productions are seldom very widely diffused, and I have heard many wish for a perusal of them. I have not translated the whole dissertation, for the former part did not strike me as new, or peculiarly excellent. I will not, by pointing out the circumstances, in which they agree or disagree with those of Spallanzani, deprive the reader of an amusing employment.

If Mr. Hunter's essay had been more remotely connected with the subject of the first Volume, I would have caused it to be reprinted. It contains so many useful views, it is so pregnant with the seeds of future discoveries in physiology, that the attention cannot be too often directed to it.

The papers of Mr. Demours and Mr. De-brow will, I hope, be read with pleasure at the end of the second Volume. They are in themselves curious, and bear a near relation to the contents.

It was my design to have annexed a translation of Reaumur's Memoirs on Digestion; but I found, that the very ample account given of them by the author, was quite sufficient for the information of the reader.

That

TRANSLATOR'S PREFACE.

That nothing might be wanting, which it was in my power to supply, I have translated the several Latin, French, and Italian passages, which it seemed proper to retain in the text.

INTRO-

INTRODUCTION.

IN the course of my public demonstrations in the year 1777, I repeated in the presence of my hearers those celebrated experiments of the Academy of Cimento, that shew the astonishing force with which the stomachs of fowls and ducks pulverize empty globules of glass in the space of a few hours. Finding them perfectly exact, I conceived the design of extending them to some other individuals of that class of birds which have been termed birds with *muscular stomachs* or *gizzards*. Such were the first lines of an undertaking, of which till that time I had never entertained the smallest idea, and which afterwards increased in proportion to the increase of my curiosity concerning a subject of so much beauty and utility as the important function of Digestion. Hence from animals with muscular, I was induced to proceed to those with intermediate, and from these to animals

with membranous stomachs (*a*). Thus I enjoyed the pleasure of extending my researches to the principal classes of animals, not neglecting Man, the noblest and most interesting of all. But these physiological researches laid me under the necessity of examining the most celebrated systems concerning Digestion, and of enquiring whether it is effected by trituration, by a solvent, by fermentation, or by an incipient putrefaction: or whether, according to the opinion of the great Boerhaave, it rather depends upon all these causes operating in conjunction. Thus I was obliged to enter anew upon a question of very ancient date, and though discussed at great length by many physiologists, yet not in my opinion sufficiently elucidated; since most writers have chosen to follow the delusive invitation of theory and hypothesis, rather than the unerring direction of decisive experiments. The impartial and judicious reader, when he shall have perused the present essay, will be able to determine, whether what I assert, is true or false.

(*a*) The I, LVIII, and CIV paragraphs will explain what is meant by birds with muscular, intermediate, and membranous stomachs.

DISSERTATION I.

C O N C E R N I N G

D I G E S T I O N.

ON THE DIGESTION OF ANIMALS WITH
MUSCULAR STOMACHS, COMMON FOWLS,
TURKEYS, DUCKS, GEESE, DOVES, PI-
GEONS.

1. **T**HOUGH there perhaps exists no animal, of which the stomach is not furnished with muscles, yet there is a singular class, justly denominated by several naturalists *animals with muscular stomachs*, since that viscus is provided in a remarkable manner with very large and powerful muscles. To this class belong fowls, ducks, pigeons, geese, partridges, &c. So great is the strength of these muscles, that many have imagined that they produce digestion by acting violently upon the contents of the stomach, and breaking down and reducing them to a pul-
taceous mass, in no respect differing from
B 2 imperfect

imperfect chyle. This notion was afterwards applied to other animals, nor was man himself exempted; and it has been pretended, that digestion is universally owing to the alternate action of the muscles of the stomach, or, as it has been termed, to *trituration*.

II. Now, to confine ourselves to animals with muscular stomachs, there was little difficulty in devising the means of determining whether the comminution and solution of food is effected by the gastric muscles. Such means have been invented and successfully applied by Reaumur. Let several animals resembling each other in structure, says that great naturalist in his two excellent memoirs on this subject, of which I shall make frequent use in the sequel, be obliged to swallow metallic tubes open at both ends, and filled with some of their natural food, as grains of the *Cerealia* when gallinaceous fowls are the subjects of our experiments. Should these grains, after they have remained a certain time in the stomach, be broken down and decomposed, we must assign a dissolving liquor as the cause of the phenomenon, since the sides of the metallic tubes must have been an insuperable obstacle to the exertions of the gastric muscles upon the contents; but if they should be retrieved in a sound and entire state, it must be acknowledged, that in these animals digestion does not depend on a solvent, but on muscular action. Such were the means employed by this sagacious naturalist. In some metallic tubes perforated at each end he enclosed grains of barley, and obliged common fowls, turkies, and ducks, to swallow them. Several hours afterwards
the

the animals were killed, and the tubes taken out of the stomach, but the barley was found quite entire; whence he inferred, that in birds of the gallinaceous class the food is not broken down by a solvent, but by strong muscular action.

III. This experiment is highly favourable to the doctrine of trituration; yet I think it would have been much more conclusive, if the same result had been obtained from other individuals of this class, and if besides barley, other grains upon which they naturally feed, such as wheat, maize, rye, chick-pease, &c. had been employed. I therefore resolved to put each of these seeds to the test of experiment in the following manner. I procured some tin tubes eight lines in length and four in diameter, and inclosed in each a number of the seeds just mentioned inversely proportional to their size. The ends of the tube were left open, but iron wires were made to pass before them, so as to cross each other, and form a kind of lattice-work. Common fowls were the first subjects of my experiments: I forced some of the tubes into the stomach, conducting them with my forefinger and thumb through the œsophagus, till I was certain they were in the cavity of that viscus. When this operation is properly performed, neither fowls nor other animals sustain any injury. In twenty-four hours the tubes were taken out, and the contents upon examination appeared to be unaltered: even the colour and taste were unchanged, if we except a slight bitter flavour which they had acquired. They had imbibed a fluid, and were a little swoln. And the same seeds inclosed in

tubes, and left in the stomach two and even three days, underwent no greater change.

IV. Sometimes, after having forced the tubes full of grains into the stomach, I immediately introduced some of the same grains loose. The latter were broken down in a few hours, but the former remained entire.

V. The food taken spontaneously by these birds does not pass immediately into the stomach, but stops for some time in the crop, where it is macerated, and becomes softer. Is such a previous maceration necessary before it can be dissolved within the tubes? This circumstance seemed to deserve attention. I therefore repeated the foregoing experiments with seeds taken from the crop of a fowl, after they had undergone a complete maceration. Notwithstanding this preparation, they underwent no change within the tubes.

VI. From these results it was easy to predict, that no new appearance would occur, if the skin should be taken off, as it really happened. It is proper to add, that other grains treated in the same manner were not dissolved.

VII. The mode hitherto practised of using tubes open at both ends, at which the gastric fluid was certainly at liberty to enter, is that of Reaumur. But this fluid having no other access, cannot exert its action on the inclosed grains so powerfully as when they are loose in the stomach, as Reaumur ingenuously confesses. To obviate this inconvenience in some measure, I had the sides perforated with a great number of holes. I had moreover recourse to another expedient. I employed hollow globules of brass half an inch in diameter, and pierced like a sieve, which I could
open

open and shut at pleasure by means of a screw worked upon the edge of the two hemispheres, into which each globule was divisible. With these new tubes and spherules I repeated the preceding experiments, not only upon common fowls, but upon ducks, turkeys, geese, doves, and pigeons: and as a larger quantity of liquor could now find its way to the inclosed substances, they were more thoroughly soaked, and had acquired a bitterer taste (III); but I could never perceive the slightest token of solution, though they continued a long time in the stomach.

VIII. These facts furnish an irrefragable proof, that the trituration of seeds in the stomach of granivorous birds, is solely owing to strong pressure and repeated and violent percussions: effects produced by the powerful muscles with which that organ is provided.

IX. The contents of the stomach are so violently agitated as to be driven in at the open ends and through the holes of the tubes and spherules, which occasions some confusion. Hence I have frequently found it of service to introduce these receivers when the stomach is empty, and to keep the animal fasting during the whole time of the experiment.

X. The strong percussion of the stomach renders another precaution highly necessary. The thickness of the tubes and spherules should be considerable, otherwise the observer, when he takes them out of the stomach, will find them broken, crushed, or distorted in a most singular manner, if they have been long retained. Reaumur mentions several accidents of this sort (*a*); and I have seen instances

(*a*) In the Memoir quoted above.

without number of such contusions, one of which I cannot forbear relating here. Having found that the tin tubes which I used for common fowls were incapable of resisting the stomach of turkeys, and not happening at that time to be provided with any tin plate of greater thickness, I tried to strengthen them by soldering to the ends two circular plates of the same metal, perforated only with a few holes for the admission of the gastric fluid. But this contrivance was ineffectual; for after the tubes had been twenty hours in the stomach of a turkey, the circular plates were driven in, and some of the tubes were broken; some compressed, and some distorted in the most irregular manner.

XI. I then tried the following means of preventing this inconvenience. Having perforated the circular laminæ in the center, I passed an iron wire through the holes, and bound it tight round the outside of the tubes; when the two ends met, they were twisted together. And now though the soldering should be destroyed, yet this contrivance would prevent the circular laminæ from receding from the ends of the tube, unless the wire passing through them should be broken. I prepared four tubes in this manner, and gave them to a turkey six months old. After they had remained a whole day in the stomach, I killed the animal; and my astonishment was extreme at finding that the tubes, in spite of my expedient, were very much damaged. All the iron wires were broken, two where they were twisted, and the two others at their entrance into the tubes: the laminæ, so far from remaining soldered to the tubes,

tubes, were found amongst the food; they were not flat as at first, but some were bent so as to form an angle, some curved, and in others, one part was pressed close to the other. The tubes had sustained equal injury; two of them were flattened as if they had been struck by an hammer, the third was moulded into the shape of a gutter, the folding of the fourth was destroyed, and it was expanded like a wafer.

XII. These phenomena will less surprize those who have learned from Redi (*a*) and Magalotti (*b*), how ducks, fowls, and pigeons pulverize hollow globules of glass in a very short space, and even solid ones in a few weeks. I have already observed, that I repeated these experiments with the greatest success (*c*). Some spherules of glass blown by the lamp, and so thick that they would seldom break when thrown on the ground, were generally reduced to small fragments, after remaining three hours in the stomachs of hens and capons; the fragments were not sharp as when they are broken by the efforts of the hand, but as obtuse as if their edges and points had been abraded by a grinding-stone. The longer the spherules continued in the stomach, the more minutely were they triturated; so that in a few hours they were reduced to a mass of particles, not larger than grains of sand. The rapidity also of this process appears in some measure proportional to the size of the animal. A wood-pi-

(*a*) Esperienze intorno a cose naturali.

(*b*) Saggio di naturali esperienze.

(*c*) In the Introduction.

geon generally breaks them less speedily than a chicken, a chicken than a capon, but a goose the soonest of all. The reason is plain, since the larger species have thicker and more powerful stomachs.

XIII. From these and other facts which I shall adduce hereafter, we may collect how much the celebrated Pozzi, formerly Professor at Bologna, was mistaken, when he considered the observations (*a*) of the Florentine Academicians, and of Redi on the power of certain animals to reduce globules of glass to pieces as false, because he failed in his attempts to repeat their experiments on pigeons. Let me here introduce an incidental remark. It is the custom of certain dabblers in philosophy to deny facts, however particularly described, and though related by persons of the highest authority, merely because their own endeavours fail of success. But they do not reflect, that this is acting in direct opposition to the principles of sound logic, by which we are taught that a thousand negative cannot destroy a single positive fact, since it is so very easy to omit some one or other of the many circumstances requisite to the success of an experiment. The Bolognian Physician has fallen into this error; instead of so rashly inferring from his own observation the falsity of the contrary event, he ought to have multiplied and varied his experiments; and if he had done this with proper precautions, he would have confirmed, instead of contradicting the relation of the Florentine philoso-

(*a*) In his short anatomical essay printed at Bologna by Lælius a Vulpe.

phers. We must suppose, that the stomachs of his pigeons were too weak and flaccid to abrade and break substances of such hardness as glass, from their being in an unhealthy state or too young; for in these cases they are by no means capable of producing such effects, as I have found from actual experience.

XIV. My celebrated countryman Vallisneri, in his judicious *anatomy of the ostrich*, proves that the hardest substances, such as stones, wood, glass, and even iron itself, are reduced to pieces in the stomach of this enormous bird by a solvent; he also inclines to think, that glass is attacked and broken by a similar liquor, which he imagines to exist in the stomachs of fowls, without the concurrence of muscular action. But the hypothesis of Vallisneri is evidently groundless, for seeds, as we have seen above, remain unaltered whenever they are defended by tubes. And when pigeons, fowls, or turkeys, are forced to swallow several balls of glass at once, some inclosed in tubes, and others naked, the latter are reduced to small fragments as usual, while the former remain entire. That the gastric muscles are the sole cause of this effect, will appear still more evidently from facts to be related in the sequel (XV).

XV. Before I proceed farther in the recital of experiments immediately relating to digestion, it may be proper to mention some other phænomena analogous to those just described. They may help to convey more distinct notions concerning this function in animals with gizzards; the smooth and blunt substances hitherto employed, could not injure the stomach.

mach. It was therefore an object of curiosity to enquire what would happen when sharp bodies were introduced. It is well known how readily broken glass will lacerate flesh. I gave a cock several fragments of a broken pane, each about the size of a pea; they were wrapped up in paper, to prevent the œsophagus from being torn as they passed through it. I was well assured that this cover would be immediately destroyed on its entrance into the stomach, and leave the glass at liberty to act with its points and edges. The animal was killed in twenty-four hours, and the glass was found in the stomach; but on this, as well as former occasions, the angles were so far obliterated, that upon putting some of the fragments on the palm of one hand, and rubbing them forcibly on the back of the other, I did not receive the least hurt. Upon weighing the glass, it appeared to have lost twenty-four grains; nor was it difficult to discover what was become of the missing particles, for the sides of the stomach, when viewed attentively, glittered with innumerable vitreous points. On the contrary, some broken bits of glass, that were inclosed in two tubes, of which one was given to a hen, and the other to a turkey, and left twenty-four hours in the stomachs, were not at all abraded at their points or edges.

XVI. Similar pieces of glass, retained two days in the stomach of a wood-pigeon, gave me an opportunity of observing other remarkable fractures and abrasions. As I have mentioned this bird, I will relate a fact very applicable to our present purpose. I gave a wood pigeon an unpolished twelve-sided garnet,
net,

net, of the size of a moderate nut, with the intention of inspecting the stomach in a few hours afterwards; the bird was confined in a cage, but made its escape by an unforeseen accident, and was confounded with many others kept in another place, so that I was not able to distinguish it; it did not fall into my hands for a month. The garnet, which had remained all this time in the stomach, filled almost its whole capacity; a circumstance which a little surprized me, since it had taken its food, and been nourished very well. But I was more surprized at finding the angles of this hard stone blunted in some places.

XVII. But the reader is impatient to know what injury the stomach received from the retention of these sharp bodies, and the violent agitation they must have undergone during the abrasion of their most pointed parts. To satisfy my own curiosity, as well as that of others, I opened the cock and two wood-pigeons (XV, XVI), and examined the internal coat of the stomach with the closest attention, after having washed away the contents. I moreover dissected it away from the nervous coat; this was easily effected: and I could now examine it to greater advantage, but notwithstanding all my pains, found it perfectly entire. No laceration, no division, not the smallest jagged appearance; it was in every respect like stomachs that had not afforded reception to any unusual substance. Only the coat of that stomach which had retained the large garnet for a month, was about three times as thick as it commonly is.

XVIII.

XVIII. Finding that these fowls sustained these experiments unhurt, I subjected them to two others far more dangerous. Twelve strong tin needles were firmly fixed in a ball of lead, the points projecting about a quarter of an inch from the surface. Thus armed, it was covered with a case of paper, and forced down the throat of a turkey. The bird retained it for a day and half without shewing the least symptom of uneasiness. Why the stomach should have received no injury from so horrid an instrument, I cannot explain: the points of the twelve needles were broken off close to the surface of the ball, except two or three of which the stumps projected a little higher. The ball had not lost its general shape, but was marked with several indentations, that certainly were not upon it at first. Two of the points of the needles were found among the food, the other ten I could not discover either in the stomach or the long tract of the intestines; and therefore concluded that they had passed out at the vent.

XIX. The second experiment, still more cruel, consisted in fixing twelve small lancets, very sharp both at the points and edges, in a similar ball of lead. They were such as I use for the dissection of small animals. The ball was given to a turkey cock, and left eighteen hours in the stomach; at the expiration of which time that organ was opened, but nothing appeared except the naked ball, the twelve lancets having been broken to pieces; I discovered three in the large intestines, pointless and mixed with the excrements; the other nine were missing, and had probably been voided at the vent.

The

The stomach was as found and entire as that which had received the needles.

XX. Two capons, of which one was subjected to the experiment with the needles, and the other with the lancets, sustained them equally well. My next wish was to know how much time elapsed before the beginning of the fractures. By repeated experiments on turkeys that were killed after intervals successively shorter, I found that these sharp bodies begin to be broken and lose their shape in about two hours. This at least happened in two individuals of the species: in one four of the lancets, and in the other three of the needles were broken within that space; the others were blunted, but continued fixed in the balls.

XXI. Let it not however be supposed, that the stomach in this class of birds is never vulnerable by sharp substances. In pullets it certainly is sometimes very much injured. I obliged two pullets to swallow some pins from which the heads had been taken. One was killed in eight, and the other in thirty-two hours. The former had not at all suffered, but two pins were implanted in the stomach of the latter. These stomachs, as well as those of many other animals, are full of rugosities, in one of which the two pins were fixed almost perpendicularly, one to the depth of a line, and the other to that of three lines: they were opposite to the most muscular part of the organ. Some force was required to extract them; at the puncture appeared a little clotted blood, with an evident livid colour around.

XXII. But whatever conclusion we are to draw from this last fact, it is certain, that the stomach of such birds is in general not subject to any injury from the introduction, residence, or trituration of these and the like substances, as I have learned from a vast variety of experiments. But how is it possible, some will enquire, that the gastric muscles can contuse, triturate, and even sometimes reduce to impalpable powder (as when glass is employed, XII, XIV, XV, XVI) these pointed bodies without injury to themselves? If the muscles act with so much force, must not the substances necessarily re-act upon the muscles? And will not this re-action cause the laceration of the internal coat of the stomach, which, though it is indeed very firm and compact, cannot sustain such violent shocks with impunity?

XXIII. This objection was immediately started, upon the discovery of the wonderful force with which digestion in poultry is effected, and an attempt was made to remove it in the following ingenious manner. It had been long known, that fowls, and other birds of the same class, have always a smaller or larger supply of little pebbles in their stomachs. It was therefore conceived, that these pebbles serve as a shield to the muscles. Hence it follows, that the comminution of bodies forced into the stomach is the immediate effect of the pebbles, and only the mediate effect of muscular action. Accordingly, the Academicians of Cimento have observed, that those ducks and fowls that contain most stones in their stomachs, soonest reduce spherules of glass to powder. Redi thinks, that the stones perform

perform the office of teeth (*a*) ; and Reaumur supposes them necessary to digestion (*b*).

XXIV. In the course of my numerous observations I can safely assert, that I never opened the stomach of a pigeon, turtle-dove, dove, partridge, fowl, turkey, goose, &c. without finding small stones within it. I have also found what is remarked by Reaumur, that the size of the stones is apparently proportional to the size of the bird. They are generally of a roundish shape, whether they acquire it from friction within the cavity of the stomach, or have it before they are swallowed. They are commonly bits of quartz, sometimes mixed with calcareous fragments. In the stomach of a turkey hen I have counted above 200, and above 1000 in that of a goose. Their existence is therefore indubitable. But is it equally certain, that they are the immediate instruments of trituration? He who is unprejudiced in favour of any theory must immediately perceive, that this is a mere hypothesis, convenient indeed and plausible, but requiring to be confirmed by experiment.

XXV. To this test I have endeavoured to bring it, and would willingly hope that I have decided the question. According to the observation of the Academicians, the birds that contain most stones, soonest triturate hard substances. Nothing was more easy than to repeat the experiment. This I did upon ducks and fowls, the two species mentioned by those learned writers, sometimes obliging them to

(*a*) L. c. (*b*) Mem. cit.

swallow globules of glass, sometimes small thin tubes of tin, and at others feeds defended by a strong cover, such as nuts of a moderate size. It was necessary that all circumstances should be alike, that the birds should be of the same species and age, and of equal vigour, &c. Not to weary the reader with too minute a detail, I shall only mention the results. In a hen and two ducks, not abundantly supplied with pebbles, the injury sustained by the substances was not so great as in three other like fowls more amply provided with them. But in four hens the effect produced was exactly the same, as far as I could judge, though the stomachs of three were less copiously furnished than that of the fourth.

XXVI. Having collected a large quantity of stones from the dissection of many gizzards, I thought they might be useful in the present inquiry; I therefore gave a certain number to some fowls and ducks, while others were left with those which they had swallowed spontaneously. The former, according to the observation of the Florentine Academicians, ought to break hard substances sooner than the latter. And so indeed it sometimes happened, but at others the event was different. Wherefore not being able to ascertain the object of my enquiry by these experiments, I had recourse to other means of solving the problem.

XXVII. The most decisive mode of determining the use of stones in digestion, evidently was to take them away altogether, either by expelling those already swallowed, or by preventing the admission of any at all. To evacuate

quate those already accumulated, it was necessary to confine the birds in cages where they could not find fresh ones, and it might be hoped, that the old ones would be gradually voided with the excrements. Accordingly, several fowls, turkies, turtle-doves, and ducks were confined separately, and that all suspicion of their picking up pebbles might be removed, the cages were raised to such an height that they could not reach the floor with their beaks. The bottoms were made of osiers placed at a distance from each other, that if the stones should pass out with the excrement they might not remain in the cage, and be swallowed again, but fall to the ground. I fed them myself the whole time, taking care that the food, consisting of corn, vetches, and maize, was free from every foreign matter, so that I was certain not a single grain of sand or the smallest stone was swallowed by them.

XXVIII. In the course of a few days I perceived some stones among the excrement, and they continued to be voided during the time of confinement. Two days before the end of the month, when they were to be killed, I forced some to swallow tubes of tin, others glass globules, some naked and some armed with needles and lancets (XVIII, XIX, XX.). I likewise gave them some grains of wheat, but did not allow them to undergo the natural process of maceration in the crop. On the 30th day every stomach was carefully examined, and though I did not completely attain the end in view, yet I gained considerable information on the subject. Not a single stomach indeed was free from stones, but they were few in number, in some instances not

amounting to above four or five, and those very small. The contusions, however, on the tin tubes, the indentations on the naked balls, the fracture of the needles and lancets, the trituration of the grain had alike taken place in every stomach; nor did it appear, that the diminution of the quantity of stones at all contributed to diminish the alteration of the several substances, or to occasion any injury to the organ that contained them. And lest it should be objected, that these hard bodies themselves performed the office of pebbles by rubbing violently against each other in consequence of the action of the gastric muscles (an objection manifestly trivial) I had taken care that each bird should not have more than one tin tube, or one glass globule, &c. These solitary substances were bruised or broken just as effectually as when many were put into one stomach; and this viscus remained as free from injury.

XXIX. Though these facts abundantly prove, that trituration does not depend on the stones swallowed by the birds in question, but upon the strength and action of the gastric muscles, I yet wished for proofs more decisive, from observing what happens in stomachs that have not received any stones. The judicious reader perceives at once, that to accomplish my purpose, it was necessary to procure young nestlings that had never been in quest of food. Accordingly, some wood-pigeons, of which the feathers had not begun to shoot, were brought me; but I was disappointed in my expectations, for even their tender stomachs were not free from pebbles, which doubtless were mixed with the food carried to them by their

their parents. Three of these young birds were sacrificed to my curiosity. The stomach of the first contained eight stones, of the second eleven, of the third fifteen; together they weighed thirty-two grains, and consisted chiefly of quartz.

XXX. As these experiments did not answer my purpose, it was necessary to take up the enquiry at an earlier period, and employ birds less advanced in growth; nay, for greater certainty such as were just quitting the egg, and therefore could not have received food from their parents. The stomach, it is obvious, could not contain stones of any kind. I was at the pains of keeping several nestlings in a warm place, while they remained unfledged, and feeding them till they were able to peck. They were then confined in a cage, and supplied at first with vetches soaked in warm water, and afterwards in a dry and hard state. In a month after they had begun to peck, hard bodies, such as tin tubes, glass globules, and fragments of broken glass were introduced with the food; care was taken that each wood-pigeon should swallow only one of these substances. In two days afterwards they were killed. Not one of the stomachs contained a single pebble, and yet the tubes were bruised and flattened, and the spherules and bits of glass blunted and broken: this happened alike to each body, nor did the smallest laceration appear on the coats of the stomach.

XXXI. I did not confine my observations to this one species. With the same view I set under a turkey-hen several eggs, partly her own, and partly of a common hen. When the chickens were hatched I took charge of

them myself, and employed the same precautions as with the wood-pigeons (XXX). They were confined for fifty-five days in separate cages, and their food consisted of various sorts of grain. The last days they had to live, I forced them to swallow the hard indigestible substances so often mentioned. Upon examination, the stomachs appeared to be free from stones, yet the fragments and spherules of glass, and the tin tubes, were not on this account either the less or the more bruised or broken. Hence then we have at length a decision of the famous question concerning the use of these pebbles, so long agitated by authors. It appears, that they are not at all necessary to the trituration of the firmest food, or the hardest foreign substance, contrary to the opinion of many anatomists and physiologists, as well ancient as modern; I will not, however, deny, that when put in motion by the gastric muscles, they are capable of producing some effect on the contents of the stomach.

XXXII. But for what purpose are they designed? If they are not necessary to the trituration of the food, are we to suppose that they contribute in any other way to digestion? Do they create a keener appetite, or maintain a better state of health, as some conceive? Are they found in the stomach because they are casually mixed with, and as it were concealed by, the food; or, because they are designedly swallowed, and even sought after?

The first questions are already answered, or rather precluded, since we have found, that birds unprovided with pebbles take their food, are nourished and grow just as well, and are

as brisk and lively as others abounding with them; an observation I have made with great satisfaction upon young pigeons, turkies, and chickens reared in the manner described above (XXX, XXXI).

XXXIII. The last question will be readily solved, if grown up chickens take their food in the same way as young ones; for these swallow every thing that comes in their way. I have often scattered amongst them various substances unfit for their nourishment, such as pebbles, bits of brick, chalk, or rubbish, which they pecked with eagerness, whether their stomach was full or empty. One day I threw before them a large quantity of the shells of the little fish, termed *Lice* by Conchologists, which they devoured till their crop was full, just as if it had been the most agreeable food. If they retain the same disposition when full grown, we may reasonably conclude, that the collecting of pebbles is less the effect of choice than stupidity; as the ostrich, according to Vallisneri and Buffon, devours without distinction whatever comes in its way, sticks, and stones, and cords, and glass, and metals, &c. such is its dulness, and so obtuse its organ of taste (a). But fowls grown to their full size, and when their natural instinct, that lay dormant while they were young, comes to be unfolded, change their manners in this as well as many other respects. A capon confined in a cage by Redi, died of hunger sooner than it would swallow pebbles offered to it in place of

(a) Buffon Hist. des Oiseaux. T. 2. Ed. in 12. Vallisn. Op. in fol. T. 1.

food (a). With me too three 'hens and one turkey, kept separately, died in the course of a few days, when instead of giving them food, I scattered before them a quantity of small stones. After their death the number of the stones was the same, though they would appear to be of the most proper kind, having been taken from the stomachs of other individuals belonging to the same species. When pebbles are mixed with the food, I have observed, that poultry, especially when hungry, pick them up. I should then incline to believe, that the stomachs of these birds generally contain a quantity of small stones; not because they are sought for and selected by design, as many suppose, but because they frequently happen to be mixed with the food.

XXXIV. Having shewn that the pebbles are not the cause of trituration, we must conclude, that it is the sole and immediate effect of the gastric muscles, which, as it is well known, are very strong, and composed of firm and compact layers, and must, therefore, when set in motion, act with great force. To be more fully satisfied of this, let the stomach of a dog, sheep, or a man be compared with the gizzard of a duck, turkey, or goose; we shall then perceive the enormous difference between the thick muscular coat of the one, and the thin one of the other.

XXXV. The internal coat, or that which immediately lines the cavity of the stomach, deserves particular attention. In many animals, and in man himself, it is soft and villous; but in gallinaceous birds it is hard and

(a) Degli Anim. viventi negli Anim. viventi.

cartila-

cartilaginous. When separated from the next, which anatomists call the nervous coat, it soon becomes dry and very hard. In turkies and geese, in which it is thicker and stronger than in common fowls, I have often dissected it away, and spreading it upon a table, have drawn along it lancets, needles, bits of glass, and such sharp substances as are triturated in the stomach without any perceptible injury to it. If indeed my hand pressed with considerable force, those parts, over which the keen bodies passed, were disunited, whether it was separated or adhered to the other coats.

XXXVI. But these substances may act in a quite different manner when under the direction of the hand, than when set in motion by the gastric muscles, and when the internal coat is not extended but forms a cavity, as it does when the stomach is entire. I therefore wished to know what happens to substances inclosed in the stomach separated from the animal, and pressed externally with the palms of both hands, and agitated in various directions. The stomach of a turkey hen was first cleared of its contents by forcing them out through the pylorus, and then a large quantity of sharp pieces of glass were introduced, which were kept in motion for a quarter of an hour by pressure and percussion on the outside of the stomach. I was in hopes, that I should thus, in some measure, imitate the natural motion. Nor was the expedient altogether ineffectual; for the internal coat was only perforated with two little holes, such as the point of a needle would have made, and yet part of the glass was reduced to powder, and part had lost its sharp

sharp edges. Different effects then are produced, when this coat is submitted to experiment after it has been removed from its natural situation, and when it adheres to the others. Nevertheless I am willing to allow, that how it should be capable of blunting and breaking the keenest bodies without sustaining any injury itself, still continues a matter of great surprize.

XXXVII. But if the inside of the gizzard be certainly agitated so violently during the trituration of the food, will not the motion be perceptible on the outside? Reaumur, induced probably by this reflection, laid open the abdomen of some of the fowls in question, and watched the stomach, but could not perceive what he perhaps imagined took place. They always seemed perfectly at rest, except the gizzard of a capon, which contracted and dilated alternately; he moreover saw certain fleshy cords moving in an undulating direction, but very slowly and gradually (a).

XXXVIII. I have perceived similar motions in two turkey cocks. Upon pressing upon the stomach forcibly with my hand, I felt a slight pulsation that produced a sensation of creeping, but was soon aware, that this was owing only to the beating of numerous little arteries, which run upon the surface of the viscus. When a perforation is made in the heart of a living animal, and a finger introduced through it, it is well known that strong pressure is felt at the time of its contraction. I made

(a) Mem. cit.

this experiment upon the gizzard of a duck, but was not sensible of the slightest compression.

Conceiving that the stomach must exert its principal action when it is irritated by substances filling its cavity, I introduced some nuts into the gizzard of a turkey hen, that had been kept fasting for a day. During the whole time I watched it attentively, through an opening made in the abdomen; when it had received only a few nuts it shewed no sign of motion, but when it was nearly full it swelled violently, and then collapsed again of a sudden. These alternations were sometimes general, and at others confined to a narrow space; they did not continue ten minutes, probably because the death of the animal was approaching from the aperture of the abdomen. The nuts were unbroken, but evident contusions appeared upon the surface. This distinct view of the motions of the stomach I ascribe to unusual good fortune, since, with the exception of one only other turkey, the stomachs of many birds of the same class remained at perfect rest, after they had been filled in the same manner. If however we consider the very morbid state of the animal when the abdomen is laid open, we shall not be much surprized at this phenomenon.

XXXIX. The various facts related in the preceding paragraphs irrefragably prove, that the food of ducks, fowls, geese, partridges, &c. must undergo the mechanical action of the gastric muscles, before it can be broken down, and reduced to an impalpable pulp. But are we to suppose, that digestion depends on this action, and that simple trituration
converts

converts the aliment into that pultaceous mass denominated *Chyme*? Or rather, that this mass is generated by means of juices either prepared or collected in the stomach; and that trituration is a co-operating, but not the immediate cause of digestion? I imagined that the tubes and spherules, which had already afforded me so much information, would not now be without their use. If the gastric juices convert into chyme the food which trituration has prepared for digestion, let some food so prepared be inclosed in the tubes and spherules, and let us see whether it will be dissolved according to this hypothesis; for it must be thoroughly soaked in these juices. I first filled a tube and spherule with crumb of wheaten bread masticated, and introduced them into the gizzard of a hen. In twenty-three hours they were taken out, and the quantity of bread was much diminished, especially at the ends of the tube, where it was also softer than at first, and had acquired a bitter taste. The same tube and spherule were forced into the gizzard of another hen, where they remained fourteen hours; after which there was no appearance of bread in either of the receivers.

XL. I repeated the experiment upon a third hen, with bread of maize instead of wheat; the tube and sphere were emptied in a day and half. As there was here no trituration nor any other power, except the action of the gastric fluid, it seemed reasonable to conclude, that this fluid had dissolved and converted the bread into chyme, and so enabled it to pass through the holes in the receivers. A doubt however suggested itself, and kept me in suspense;

pense; without supposing the transmutation of the bread into chyme, the gastric fluid by merely diluting it, like water, might render it capable of passing out of the tubes and spherules.

XLI. A substance not soluble by simple maceration, and at the same time softer than grain, upon which the gastric juices have no action (III, IV, V, VI, VII.), was wanting to clear up the doubt. Flesh seemed to correspond to this description. Flesh is digested by many birds with gizzards, which for the most part are both frugivorous and granivorous; I therefore filled four tubes with some veal (*a*) bruised very small to supply the want of trituration, and forced them into the stomach of a hen. They were taken out in twenty-four hours, and the flesh was in the following state: In the tube that came first to my hands it did not amount to above one-twentieth of its original bulk, in two others it had suffered nearly the same diminution; the only difference appeared in the fourth, which was not open at both extremities like the other three, but closed at one end with a circular plate of iron. The flesh contiguous to the plate preserved its red colour and natural consistence, and did not seem at all wasted; but at the open end it was reduced to two thirds of the length of the tube, of which it had at first occupied the whole; the part that continued firm and red retained the true flavour of flesh; at the opposite end it had entirely lost that flavour, and the surface, to the depth

(*a*) Wherever I mention flesh without an epithet, I mean raw flesh.

of a full line, was besides reduced to a pulp, and had acquired a cineritious colour. The inconsiderable residuums in the other tubes were altered in the same manner.

The immediate consequences of these experiments are self-evident. The remarkable diminution of the flesh arose from its having been in great measure dissolved and digested; for all physiologists agree in considering the change of colour and taste, and the transmutation of the food to a pultaceous mass in the stomach, as the characteristic marks of digestion. The three tubes, of which the sides were perforated and the ends open, admitted the gastric liquor at every part. Hence the remarkable waste of the flesh in them. The case was different in the tube closed at one extremity, and nothing can be more obvious than the reason; for as the liquor could only enter at one end, it could only there dissolve the flesh.

XLII. This experiment decisively proves, that the gastric liquor was the cause of digestion in the present instance; and it was easy to foresee, that others upon the same class of birds would be attended with the same result. Some tubes filled with flesh were next introduced into the gizzard of a very large turkey cock, but the lattice work at the open ends, though it consisted of iron, could ill withstand the action of such powerful muscles. Upon examination seven hours afterwards, it was found separated from the tubes, and coiled up in one mass near the pylorus, in the midst of the pebbles and scoriæ of the food, some of which were jammed so closely in the tubes, that there was some difficulty in forcing them out with

the point of a penknife. I could not perceive the smallest fragment of flesh amongst them, and remained in doubt whether it had been digested, or expelled by these extraneous bodies. I resolved to submit this species of bird to further experiments, but was obliged to abandon the tubes, and have recourse to the hollow spherules, of which I have spoken above (VII). They were made thick and strong, with many small pores over the whole surface, in order to obviate two inconveniencies, the one lest the receivers should be unable to resist the violent impulses of the stomach, the other to prevent the matters compressed and agitated by the action of the muscles from entering so readily into them. Two of these spheres were given to a turkey cock eleven months old, and in twenty-four hours extracted from the gizzard. They contained at first about twenty-eight grains each of beef and veal bruised very small. Upon opening them after the same interval as before, and weighing the flesh, the beef was found to have lost nine, and the veal thirteen grains. I must not however omit to remark, that they were both fully impregnated with gastric liquor, and consequently would have weighed still less if they had been free from it. The beef and veal, when touched with the point of a knife, seemed tenderer than in their natural state, and resembled a soft paste rather than flesh. They had the bitter taste of the gastric juice with which they were impregnated, and the colour approached more to white than red. They were replaced in the spheres, and kept twelve hours in the gizzard of another turkey-cock.

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Upon a fresh examination, the beef weighed only eight, and the veal only five grains. The gastric fluid had therefore produced a new solution, and this process was entirely completed after the spheres, into which the flesh was put for the third time, had continued five hours in the stomach of a third turkey-cock.

XLIII. Flesh is digested by the gastric liquor of geese as well as of turkies. Eleven grains of beef, inclosed in a spherule, were entirely dissolved in two days in the gizzard of one of these large birds.

I will not describe three other results obtained, one from an hen, and the two others from two capons; since, with respect to the digestion of the flesh, they are exactly like those just mentioned.

All these experiments were made with flesh bruised very small; this condition is not indeed indispensably requisite, but it very much promotes digestion. The bruised flesh was always dissolved in two days, but when entire that process was not completed in four, and sometimes not even in five days. The reason of this difference is obvious. The more flesh is bruised, the larger surface does it acquire; and in proportion to the increase of surface, more points are exposed to the action of the gastric liquor, which will consequently sooner complete the solution.

XLIV. Before I proceed further and conclude the present dissertation, I must notice an experiment of Reaumur, which does not perfectly agree with those just related. The greatest part of his memoir is employed in shewing the great force of the gizzard of gallinaceous fowls in triturating the food; in
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the remainder he endeavours to prove, that this viscus contains no menstruum of sufficient efficacy to produce solution. In support of this proposition, besides the argument derived from barley continuing unaltered within the tubes, he adduces the following fact, which requires to be particularly related. It is well known, how greedily ducks devour, and how soon they digest, flesh. In order therefore to obtain the information he wanted, Reaumur had recourse to this bird: Having provided six tubes, four of lead, and two of tin, he inclosed in the former bits of veal of the size of a grain of barley, and in the latter some considerably larger. These six tubes he gave to a duck at different times; viz. a leaden one at ten o'clock in the morning, and another at eight in the evening; next day a third was given at six in the morning, together with the two tin tubes; lastly, at nine the same morning the animal was made to swallow the last leaden tube, and at ten was killed. Of the four leaden tubes, one was voided the preceding day at nine in the evening; it was that which had been taken at ten in the morning; the other five remained in the gizzard, and the flesh was not only entire, but as firm as at first. Some of the pieces retained their red colour, three of them however had lost it. The whole capacity of some of the tubes was no longer filled by the flesh; not that it had suffered any diminution, but because it was compressed by the stones and food, which had been admitted at the open ends of the tubes. From this experiment Reaumur infers, that no menstruum had acted on the flesh, since it was not either com-

minuted or dissolved. And though he does not affirm, that in the gallinaceous class digestion is the effect of trituration alone, he yet concludes, that the gizzard contains no solvent capable of decomposing and digesting the aliment.

XLV. What has been above related, shews how far Reaumur's conclusion ought to be extended; when we speak of aliment of a hard and compact texture, such as seeds, it must be allowed, that the gastric liquor has no action upon them (II, III, IV, V, VI, VII.); but when we are considering food naturally tender, like flesh, or such as is made so by art, like grain in the form of masticated bread, it must then be allowed, that a perfect solution is effected by the gastric juices alone (XXXIX, XL, XLI, XLII, XLIII.). In Reaumur's experiment the flesh remained so short a time in the gizzard, that we cannot be surprized if it was not sensibly dissolved. If we attend to the times at which he gave his tubes to the duck, and at which he killed it, we shall immediately perceive, that the tube which continued longest in the gizzard, remained in it only twenty-four hours; a space insufficient, according to my experiments on fowls, turkies, and geese (XLI, XLII, XLIII.), for the gastric liquor of these birds to dissolve any sensible portion of flesh inclosed in tubes. I should however have condemned myself for a crime of omission if, to the proof deducible from analogy, I had neglected to add direct experiments on ducks. Upon two ducks therefore I repeated the experiment of the French Naturalist, with the following variation; four tubes, each containing

taining a bit of veal equal in size to a barley-corn, were given to a duck; in two of the tubes the flesh was whole, but in the two others it had been previously cut into small bits: in fourteen hours the gizzard was examined; the four tubes were found in it; the two entire pieces of flesh were of their original size, but inclining to a white colour; the small bits were also about the same size as at first, but were converted into a gelatinous paste. The experiment was repeated upon another duck, which was not killed till the end of the second day; and now the tubes that had contained the minute bits of flesh were entirely empty; and in the others, only some slight traces of a gelatinous concocted matter remained. If we combine these facts with others before related, it will appear, that in the gallinaceous class, trituration and the gastric fluid mutually assist each other in performing the great function of digestion; the former by breaking down the aliment, acts as the pre-disposing cause; the latter, when it is thus prepared, penetrates into it, destroys the texture, dissolves the particles, and disposes them to change their nature, and to become animalized.

XLVI. But what is the origin of this gastric fluid, so useful in digestion? How is it mixed with the food? And what successive changes does the latter undergo from the action of trituration, joined to that of the gastric liquor? These important questions required a strict examination of the œsophagus and gizzard, as also of the food during its passage through, and continuance in, these parts. As experiments are more conclusive,

the larger the scale is, on which they are conducted, I conceived that the larger species, as geese, turkies, ducks, and fowls, would be the best subjects for these enquiries. To begin then with the œsophagus of a goose, this canal at the end towards the mouth, has the appearance of an inflated intestine; it is above a foot long, and at its origin about an inch in diameter, but widens as it descends, for the space of six inches and more, when it contracts like a funnel, then enlarges again, and this enlargement continues to the gizzard. The œsophagus is membranous, its sides are strong and thick; they are thickest at three inches distance from the stomach, on account of a fleshy fascia, of which I shall speak below. If we look very attentively, we can perceive the whole œsophagus covered with points or elongated spots, which are most numerous just above the funnel. The fascia appears to consist of a multitude of cylindrical bodies, larger than husked millet-seed. These bodies shine through a fine membrane, externally surrounding the fascia.

XLVII. If the œsophagus be inverted, and the spots examined by the help of a glass, we plainly perceive that they are follicular glands. This likewise is confirmed by the appearance of moisture on the œsophagus, when they are pressed. But the follicular glands that appear through the fleshy fascia like cylindrical bodies, bigger than husked millet, as we before observed (XLVI), are far more easily distinguishable, because far larger. This fascia, which encircles the œsophagus like a ring, is above an inch in breadth, and about a line in thickness. Great part of it is invested by a thin covering

covering of a deep yellow colour, and consequently very liable to be torn. When this is removed, the fascia externally appears white and rough, on account of the numberless prominent papillæ, each of which has an evident pore in the center. When the fascia is stretched, and much more when it is pressed between the fingers, a drop of whitish turbid liquor gushes out at each pore into the œsophagus; and it may be enlarged, by continuing the dilatation or pressure. The liquor is dense, somewhat viscid, of a sweetish, and at the same time saltish taste. To comprehend immediately that the pores are the excretory ducts of the follicular glands lying below, requires very slight anatomical knowledge: the glands appear very distinctly, when the membrane in which the pores are inserted, is removed. The follicles are of a pale red colour, and full of a turbid liquor, which oozes out from the excretory ducts, when the œsophagus is kept under water.

XLVIII. Below the fleshy fascia, the œsophagus becomes membranous again for nearly the breadth of three quarters of an inch, when it is inserted into the gizzard. This organ is of the size of the fist, remarkably hard, and of an irregular elliptical figure; when opened lengthwise at the thinnest part, it is divided into two large muscles, each above an inch in thickness, and composed of very compact fibres. It appears plainly, that the whole action of these great muscles consists in approximating with violence, and like the sides of a vice, crushing and breaking to pieces all interposed substances. As the nervous coat adheres to these strong muscles,

and as, however robust, it might be injured by such impetuous shocks, nature has sagaciously invested it with a cartilaginous coat, of a structure more capable of resistance, which internally lines the cavity of the gizzard.

XLIX. In turkies the œsophagus and stomach very nearly resemble those parts in geese. The former, however, is more membranous, and abounds more in follicular glands of a larger size, and consequently more conspicuous. The excretory ducts may be easily seen, and the liquor of the follicles may be readily forced out by pressure. This liquor is transparent, and somewhat viscid; its taste is rather sweet. But the œsophagus of the turkey has one peculiarity not found in the goose; it is provided with a bursa or bladder, well known under the name of the *crop* or *craw*. In this species it is very large. The crop at the sides at least, if not at every part, is furnished with follicular glands, exactly like the others. At the lower part of the œsophagus we also find the fleshy fascia, an inch in breadth, and provided with follicles much larger than those of the crop or œsophagus, and in great abundance. The liquor seems to have the same properties as in the goose. It is viscid, has a sweetish and saltish taste, a turbid white colour, and considerable density.

The gizzard, whether its form or the nature of its coats be considered, is exactly like that of the goose, only weaker and smaller in proportion to the inferior size of the bird.

L. I have observed all that has been related with respect to the gizzard and follicular glands of the goose and turkey in due proportion in the duck, common fowl, and even in smaller birds of the same class, as the pigeon, partridge, wood pigeon, turtle-dove, and quail; with this peculiarity only, that in the duck the œsophagus, instead of forming a crop, has the same structure as in the goose (XLVI). I shall therefore omit a description of these parts, and proceed to consider the stomach in a physiological light.

LI. In speaking of this organ, I have never mentioned either follicles or glands; for in the fowls hitherto mentioned, I could never discover any. The internal coat, from its cartilaginous nature, appears to be unfit for the insertion of glandular bodies; at least I was not able to find the smallest vestige of them; nor did I succeed any better in the nervous or muscular coats, notwithstanding I examined them very narrowly. Reaumur having observed a vast number of short white filaments between the cartilaginous and nervous coats, entertained some suspicion of their being tubes or vessels, placed there in order to discharge their contents into the stomach (*a*). I have found these filaments in all the gallinaceous fowls I have examined; but cannot agree with him that they remain attached to the nervous, when the cartilaginous coat is separated from it: for after such separation, I have ever seen them adhere to the cartilaginous, never to the nervous coat; but any person may readily make the trial. These

(*a*) Mem. Cit.

filaments are very numerous; they are pointed at the extremity, opposite to that which is inserted into the last mentioned coat, and resemble short white down, distinctly visible by the naked eye in the larger birds; such as the goose and turkey, but requiring the aid of a glass to be seen in the smaller species. I have divided many of various sizes with the points of very fine needles, in order to discover whether they were hollow or glandular, but could never find any appearance of this kind: I have also squeezed them in order to see if any liquor would ooze out, but to no purpose: and so far from suspecting these filaments of Reaumur to be vascular or glandular, I should rather suppose them to be merely for the purpose of joining, or at least more closely connecting the cartilaginous with the nervous coat.

We shall see elsewhere, that stomachs of the membranous kind, when they are taken out of the animal and rubbed dry, soon become moist again: this moisture comes from invisible vessels and glands, discharging their liquor into the cavity of the stomach. I have made the same experiment on muscular stomachs, but they always continued dry; the same thing also took place, when I pressed them underneath, though this is a very effectual means of accelerating and increasing the aqueous covering. Hence I have good grounds for supposing, that the juices found in muscular stomachs do not properly belong to them, but come chiefly from the œsophagus, and in part from the duodenum, as we shall see below.

LII. Nature however, has not failed to provide the quantity necessary for digestion. We have seen the vast number of follicular glands with which the œsophagus is provided (XLVI, XLVII, XLVIII, XLIX.); they must needs pour in their liquor in great abundance. And experience confirms what reason suggests. I introduced a small piece of dry sponge, previously cleansed from every impurity, into the craw of a pigeon, in which it was left twelve hours; at the expiration of this time I opened the craw, and took it out. The sponge was full of liquor, and on being squeezed over a glass, afforded above an ounce. I employed larger pieces of sponge in fowls and turkies, and obtained more of this œsophageal liquor; the quantity in a turkey amounted to seven ounces in ten hours. A similar liquor is procured in equal abundance, from such œsophaguses as are dilated into a large canal, instead of a craw, as in ducks and geese (XLVI, L.). This fluid is undoubtedly designed to soften the food which remains a certain time in the craw, or in the large canal; and this not only disposes it to be more readily broken down, but very probably also communicates to it some quality that renders it more easily digestible. But it is likewise certain, as I have found from experiment, that a considerable part of this fluid descends into the stomach; not to mention that denser and more viscid liquor which distils from the fleshy fascia, lying at the bottom of the œsophagus, (XLVI, XLVII.).

LIII. These various œsophageal juices acquire in the stomach a bitter flavour, resembling that of the food in this viscus: and as
this

this taste exactly resembles that of bile, which in these animals is discharged through the cystic duct into the duodenum, I am thoroughly persuaded that it arises from this source, in consequence of the bile regurgitating into the cavity of the stomach, and being mixed with the food and œsophageal liquors collected there. I am confirmed in this persuasion by other facts, which I shall have a more convenient place for relating; not to mention the well known circumstance of the bile being found in the stomach of various animals (a).

LIV. This collection of divers liquors in the gizzard of our fowls, serves as a menstruum for the food, and for disposing it to be transmuted into chyle. But the first step towards this event is taken in the craw. It is here that the aliment is penetrated by the œsophageal liquor, and begins to change its smell and taste: that of the hardest texture is prepared to be broken down when it descends into the stomach, which in these birds may be said to supply the place of teeth.

But the way in which the food descends from the mouth into the stomach, is deserving of attention. When our fowls are abundantly supplied with meat, they soon fill their craw: but it does not immediately pass hence into the gizzard, where it does not arrive till after it has been macerated in the craw: it always enters in very small quantity, proportional to the progress of trituration in the stomach. Here then, what happens

(a) Haller Elem. Physiol. T. 6. Vallisn. Op. in Fol. T. 1.

happens in a mill, may be observed to take place. A receiver is immoveably fixed above the two large stones which serve for grinding the corn; this receiver lets the corn which it contains, fall continually in small quantity into the central hole in the upper stone, through which it passes, and diffuses itself in the void space between the two stones, where it is broken down, triturated, and pulverized by means of the strong friction of the upper stone that moves round with great velocity upon that below. Meanwhile the flour passes from between the stones, as substances triturated by the gizzard, and dissolved by the gastric juices, are expelled through the pylorus into the small intestines.

LV. All this may be observed, by inspecting the alimentary canal during the time of digestion. If the bird has fed upon grains, they are found in the cavity of the gizzard, partly entire, but softened by a fluid. That part of the œsophagus that lies between the end of the crop and the beginning of the stomach, either contains no grains at all, or only a few quite entire. Trituration takes place in the gizzard only. Those which have first entered this cavity, are found to have lost the farinaceous substance, and are reduced to mere bran; the succeeding ones are more or less broken, and the last are entire. Amid this mixture of bran, and broken and entire grains, we always find a semi-fluid pultaceous mass of a whitish yellow colour. This is the farinaceous part of the grains decomposed by the gastric liquor, and converted into chyme. Meanwhile fresh grains continue to fall into the gizzard, in order

order to undergo the same transmutation: this admirable process continues as long as the grains continue to fall into the stomach.

These appearances and changes take place also in animal substances, whenever birds with muscular stomachs feed upon them.

LVI. At whatever time the stomachs of these birds happen to be opened, they always contain a certain quantity of gastric liquor. This is less abundant when they are full of food, (for in this case it is absorbed by the food) than when they have little or none. If we wish therefore to be provided with a large quantity of this liquor for experiments, it should be taken from the empty stomach. Besides, in this case it is purer than when mixed with the food. When examined in a state of purity, its transparency, if we except a slight yellow tinge, is little inferior to that of water. It has likewise the fluidity, but not the insipidity of water, being always a little bitter, as well as salt. I have found that the gizzards of turkies and geese most abounding in gastric juices, probably on account of their superior size. I was induced by the quantity they afforded to attempt an experiment, which if it succeeded, would still further prove that trituration is only an assisting or predisposing, and not the efficient cause of digestion. It consisted in trying, whether these juices retain their solvent power out of the stomach. For this purpose, I took two tubes sealed hermetically at one end, and at the other with wax: into one I put several bits of mutton, and into the other several bruised grains of wheat, and then filled them with the gastric liquor. In order that

that they might have the condition which in these animals precedes digestion, they had been macerated in the craw of a turkey cock. And as the warmth of the stomach is probably another condition necessary to the solution of food, I contrived to supply it by communicating to the tubes a degree of heat nearly equal, by fixing them under my axillæ. In this situation I kept them at different intervals for three days, at the expiration of which time I opened them. The tube with the grains of wheat was first examined; most of them now consisted of the bare husk, the flour having been extracted, and forming a thick grey sediment at the bottom of the tube. The flesh in the other tube was in great measure dissolved, (it did not exhale the least putrid smell) and was incorporated with the gastric juice, which was hence rendered more turbid and dense. What little remained had lost its natural redness, and was become exceedingly tender. Upon putting it into another tube, and adding fresh gastric liquor, and replacing it under the axilla, the remainder was dissolved in the course of a day.

I repeated these experiments with other grains of wheat bruised and macerated in the same manner, and likewise upon some flesh of the same kind, but instead of gastric juice I employed common water. After the two tubes had remained three days under my axillæ, I found that the grains, where they were broken, were slightly excavated, which was occasioned by an incipient solution of the pulpy substance. The flesh had also undergone a slight superficial solution, but
internally

internally it appeared fibrous, red, firm, and in short, had all the characters of flesh. It was also putrid; the wheat too had acquired some acidity, two circumstances which did not take place in the grains and flesh immersed in the gastric liquor. These facts are then irrefragable proofs that the gastric juice retains even out of its natural situation the power of dissolving animal and vegetable substances in a degree far superior to water.

LVII. The gastric juice which I employed was taken from a turkey. That of a goose produced similar effects. I have further found, that in order to obtain the solution of animal and vegetable substances, this juice should be fresh. It loses its efficacy, when it has been kept some time in vessels, especially if they should happen to be open. It also becomes inefficacious after it has been used for an experiment. Lastly, a considerable degree of heat, equal to the temperature of man or birds, must be applied; otherwise, the gastric juices are not more effectual in dissolving flesh and vegetables than common water. This artificial mode of digestion is well calculated to illustrate the subject I have undertaken to treat; but I shall have opportunities of speaking of it at greater length in the subsequent dissertations.

DISSERTATION II.

CONCERNING THE DIGESTION OF ANIMALS WITH AN INTERMEDIATE STOMACH. CROWS. HERONS.

LVIII. **B**Y the term intermediate stomach, I mean such a stomach as, on the one hand, is not properly muscular, that is, provided with thick and strong sides, as in the gallinaceous family (I.); and on the other, is not merely membranous, that is, very thin, as in birds of prey and man, but has an intermediate degree of thickness and strength. The stomachs of both the raven (*a*) and grey crow (*b*) may be considered in this light, though in reality they approach nearer to the muscular than the membranous class. The intermediate power of these stomachs contributes also to characterize them; it is

(*a*) These two species are called by Linnæus, *Corvus cinerascens*, capite, jugulo, alis caudaque nigris.

Corvus ater, dorso atro-cœrulefcente, cauda subrotunda.

(*b*) The hooded crow of Pennant. *Corvus Cornix* L.

very

very far from being equal to the force of muscular, but greatly exceeds that of membranous stomachs. Such tubes of tin, as doves and pigeons would flatten and disfigure with the greatest ease, remain unaltered in the stomach of crows. Thus also grain is triturated by the former, but continues whole in the latter. Their gastric muscles however are not inert. They exert a certain degree of action, but it is far inferior to that of the gizzard in the gallinaceous class. Thus, though they cannot compress tin tubes, they are capable of producing this effect upon tubes of lead, provided they are very thin: and those that continue unaltered at first, are at length slightly incurvated or distorted at the edges, and generally filled with fragments of the food, evident marks of considerable motion in the gastric muscles; there are no effects which shew such motion in animals with membranous stomachs, as we shall find in its proper place. I have often seen these phænomena, having kept a great number of grey crows and ravens: how useful they have been in the course of my enquiries the reader will learn from a perusal of the present dissertation.

LIX. These birds, as well as man, may be denominated *omnivorous*. Herbs, grass, leguminous seeds, flesh of every kind, alive or dead, serve equally for their nourishment. As these two species possess powers for the concoction of various aliments, either entirely the same as, or strongly resembling those of man, it is obvious, that the knowledge obtained from them will greatly illustrate the process

process of digestion in us. They besides seem formed on purpose to forward the views of the observer. When we wish to know what changes have been produced in substances inclosed in spheres and tubes, and given to gallinaceous birds, it is necessary to extract the tubes and spheres from their gizzards; that is, it is necessary to kill them. Hence for every experiment we must sacrifice an individual, at no small expence to our philosophical curiosity. On the contrary, we can perform such experiments upon crows as often as we please, without destroying a single individual. With respect to substances, they are incapable of digesting such as the above-mentioned metallic receivers I have discovered, that they possess the privilege of returning them through the mouth, as birds of prey vomit the feathers and hair of the animals they have devoured, a circumstance well known both to naturalists, and those who train falcons for the field. But whereas this vomiting generally takes place every twenty-four hours in birds of prey, in crows it happens at least every nine, and commonly every two or three hours.

LX. As I obtained the same results from both species, I will employ in my narration the generic name only (*a*). My observations were begun in winter, the most convenient season for procuring a large number, owing to the multitudes, especially of ravens, with

(*a*) *Corvus* is the generic name in Latin, and *Cornaccia* in Italian, and *Crow* may very well serve for it in English.

which Austrian Lombardy, and indeed almost all Italy then abounds. All the crows which I could obtain, when newly taken, had a large collection of pebbles in the stomach; the biggest were of the size of small pease, the least of that of millet: they were of various sorts; I even found rounded pieces of brick. But in less than ten days not a stone remained in the body, a circumstance which I learned from the inspection of several stomachs, when I had occasion to kill some crows in order to observe the anatomical structure of the alimentary canal. They were voided partly at the anus, as it appeared from the excrements, and partly through the mouth; in the latter instances they were glued by the gastric liquor to the outside of the tubes which I had forced them to swallow, and which they afterwards threw up. When unprovided with pebbles, they continued to eat and were nourished as well as before. Hence it is to be inferred, that they are not more necessary to digestion in birds with intermediate, than in those with muscular stomachs, as we have seen above (XXXI). And as I inclined to believe, that the last-mentioned class do not pick up these stones from choice, but mere accident (XXXIII); so I consider the matter likewise with respect to crows, having observed, that though unprovided, they never peck them eagerly, even when hungry, but swallow them only when they happen to be mixed on purpose or by chance with their food, and as it were concealed by it.

LXI. I began my experiments by putting entire seeds in the tubes (a). These seeds were beans and wheat. The reader will easily perceive, that crows are not so stupid as to take the tubes spontaneously, but that it is necessary to force them down the throat, and to pass the finger along with them till they are got into the stomach. This I executed in the way I had before done in animals with muscular stomachs (III). The tubes were all thrown up in the space of three hours. The beans and wheat appeared as at first, excepting that they were somewhat softened and swelled by the gastric juice, which had penetrated some way into them. I replaced the grains in the tubes, and introduced them again into the stomach, where they remained two hours longer, without undergoing any further change. I repeated the same experiment a great number of times, and upon computing the space during which the tubes had continued in the stomach, I found that it amounted to forty-eight hours; in this interval the seeds had suffered no other alteration, except being a little more moistened. The gastric fluid is therefore incapable of effecting the solution of these vegetable matters.

LXII. But we have before said, that they were entire; on which account, this juice could not act upon the farcinaceous substance of the grain till it had traversed the husk; and this might have diminished its efficacy. In order to determine how far the suspicion was well-founded, it became necessary to re-

(a) These tubes were the same I used for gallinaceous fowls, and I continued to employ them in the sequel.

peat the experiment upon the same seeds bruised. This was done, and four tubes full of the coarse flour were given to a crow: they remained eight hours in the stomach, and proved the justness of my suspicion; for upon examining the contents, I found above a fourth part wanting. This could arise from no other cause but solution in the gastric liquor, with which the remainder was fully impregnated. Another observation concurred in proving the same proposition: the largest bits of wheat and bean were evidently much diminished; this must have been owing to the gastric liquor having corroded and dissolved good part of them, as the nitrous acid diluted with a large quantity of water, gradually consumes calcareous substances. I replaced what remained of the seeds in the tubes, and committed them again to the stomach, wherein they remained, at different intervals, twenty-one hours; at the end of which period they were entirely dissolved, nothing being left but some pieces of husk and a few inconsiderable fragments of the seeds.

LXIII. Wheat and beans floating loose in the cavity of the stomach, undergo the same alteration as in the tubes. When I fed my crows with these seeds, I observed, that before they swallowed them they set them under their feet, and reduced them to pieces by repeated strokes of their long and heavy beaks. And now they digested them very well; nay, this process was very rapid in comparison of that which took place within the tubes. But when the birds either from excessive hunger or violence swallowed the seeds entire, the greatest part of them passed out entire

tire at the anus, or were vomited. We cannot therefore be surprized, that the gastric juice could not dissolve them within the tubes, since it was incapable of effecting this process within the cavity of the stomach, where its solvent power is far superior.

LXIV. To avoid being tedious, I will not speak of other feeds submitted to the same experiments; such as chicken-pease, French beans, pease, and nut-kernels. I will rather mention vegetable matters of a softer and more yielding texture, which did not require to be broken in order to be dissolved; such as crumb of bread and apples. These substances are not only dissolved within the tubes, but require a much shorter time than beans and wheat. Several bits of a ripe apple, weighing eighty-two grains, were put into some tubes, and dissolved in the space of twenty-four hours in the stomach of a crow. Four bits of another apple, weighing an hundred and three grains, were dissolved in little more than fifteen hours. Of an hundred and seven grains of crumb of wheaten bread, there only remained eleven after the space of thirteen hours.

LXV. From vegetable I proceeded to animal substances. The greediness which crows shew for these afforded a certain presage, that they would be dissolved within the tubes. I filled eight tubes with beef, and gave them to four crows, two to each. The flesh was not minutely bruised, as in the case of gallinaceous fowls (XLII), but each tube contained an entire piece. An hour had scarce elapsed when one was thrown up. The flesh, upon examination, did not appear to be sensibly

sibly diminished, but it was thoroughly soaked in the gastric liquor. The juice was a little bitter, and of a yellowish green colour; the flesh had acquired the same taste and colour in several places. In an hour and three quarters two other tubes were vomited up; and now the flesh began to shew marks of solution. The red colour was changed to a dark cineritious hue, and the whole surface was become flabby, and the cohesion of the parts was destroyed. In another tube, discharged after the space of two hours and an half, the solution had made a greater progress. A dark covering of jelly surrounded the flesh, which came away when it was touched with the fingers; when applied to the tongue, it hardly exhibited the flavour of flesh. The solution had proceeded still further in four hours. At the expiration of that time two other tubes were thrown up; in these the flesh did not amount to half the original quantity. The remainder was surrounded by the same gelatinous covering, under which it preserved its natural colour, fibrous structure, and savour. There remained only two tubes, which were vomited up seven hours after they had been taken. Both were empty, the flesh therefore had been completely dissolved, except a few bits of jelly that adhered to the inside. I never could perceive the smallest token of putrefaction either during the progress, or at the completion of the solution. And this observation, that I may not be under the necessity of repeating it continually, is to be extended to all the solutions performed not only by other crows, but by all the animals that

that I shall have occasion to mention in this work; for I can assert with the utmost confidence, that I have never been sensible of the slightest stench either in flesh or any other substance which I introduced in tubes or any other way.

Nothing could be more satisfactory than the information obtained from this experiment. It not only rigorously demonstrates, that the gastric liquor of crows is the solvent of flesh inclosed in tubes without borrowing the least aid from trituration, but it throws still stronger light upon the mode of operation of this menstruum in the gallinaceous class. It begins by softening the texture and altering the colour; next succeeds the decomposition of the parts; this transmutes the flesh into a kind of jelly of a taste different from that of flesh: the jelly is then more thoroughly penetrated by the juice and extracted out of the tubes, and in the stomach is changed into chyle. It appears also, that this fluid does not penetrate deeply into the flesh, but acts on the surface only, dissolving and removing one layer at a time, if we may so speak, like other corroding menstrua, until it comes to the innermost part, which it also softens and melts.

LXVI. We have seen, that the flesh in the tubes shewed no sign of solution till an hour and three quarters had elapsed, and that this process was completed at the end of seven hours (LXV). But are we to conclude, that this is the measure of the time required by the gastric liquor for this operation? or that it would have been accomplished in a shorter time, if the liquor had had free access to the flesh?

flesh? for it is certain, that the tubes are no small impediment to the gastric juice. What then would happen if the impediment was in part removed? and what when it is entirely taken away, by putting the flesh loose into the stomach? In order to solve the first of these interesting questions, I enlarged the perforations in the sides of the tubes as much as possible (VII), then filled them with beef, as before (LXV), and introduced them into the stomachs of several crows. I now perceived with pleasure the superior efficacy of the gastric liquor. In an hour and an half three of the tubes were thrown up, and above a fourth of the flesh appeared to be wasted. Two other tubes were discharged in less than two hours, and contained little more than half of their original quantity. And before the completion of the fourth hour, the remaining tubes were entirely empty.

LXVII. Before I proceeded to the other question, I thought of inverting the foregoing experiment (LXVI), and instead of allowing freer access to the gastric juice, of impeding it more and more, and at last hindering it almost entirely. I began with employing the usual tubes wrapped in cloth; this, although it was thin, was sufficient to prevent the solution of the flesh, which now did not begin to take place till three hours after the tubes were introduced into the stomach, and was not completed till ten had elapsed.

The linen in which the tubes had been wrapped was single, I now doubled it in order to hinder the ingress of the liquor more effectually, and repeated the experiment in the

the same manner. The flesh afforded no token of solution for four hours, and was not entirely dissolved for a whole day.

Upon wrapping round another fold the solution did not begin till nine hours had elapsed, and at the end of a day the flesh was scarce half consumed. In other respects the gastric juice had acted upon the flesh just as it does in open tubes, excepting only the slowness of its operation. It was become externally gelatinous, and incoherent in its parts. It was tinged yellow in several places; the taste and smell at the surface were not different from those of the gastric liquor.

I concluded these experiments, by trying what would be the effect of putting flesh into tubes with only three or four holes. After they had continued nine hours in the stomach, the result was as follows: small excavations of greater or less depth were made in the parts opposite to the pores, and from these excavations small furrows wandered irregularly along the surface of the flesh. The fleshy fibres both in the cavities and furrows were become exceedingly tender, they had besides lost their red colour, and were turned yellow. The rest of the flesh was unaltered. From what has been said before, the origin of the cavities and furrows evidently appears to have been derived from the gastric juice, which by insinuating itself through the little perforations, had there dissolved and destroyed the flesh; the rest remained entire, because none of the juice could enter, if we except a very slender stream, which had produced the furrows.

LXVIII. Let us now proceed to the second question, by which we are led to examine how much more readily flesh lying loose in the stomach is digested than when it is enclosed in tubes. Taking some of the same kind of flesh that had been used before, viz. beef, I parted it into two equal portions, one of which was again divided into smaller bits before it was put into the tubes, while the other was left entire. Each portion weighed eleven pennyweights. I next gave the tubes, which were eight in number, to a crow, and to another bird of the same species, equally healthy and robust, I gave at the same time the whole portion of flesh, to which I had previously fastened a thread. This thread, hanging out of the bird's mouth, and being wrapped round the neck, I could draw up and examine the flesh at pleasure. And that every circumstance might be alike, I had taken care that the two crows should have their stomachs empty. In thirty-six minutes one of the tubes was vomited, and at the same instant I drew up the flesh from the stomach of the other crow. The latter was thoroughly imbibed with gastric juice, especially the part that rested upon the bottom of the stomach. It had lost its redness, and was now of a dirty colour; it weighed forty-two grains less than at first; on the contrary, the flesh enclosed in the tube retained its original weight.

The tube and the flesh tied' to the string, were replaced in their respective situations; and in order that both might remain the same length of time in the stomach, I took care to return the tubes as they were

were thrown up. The flesh was entirely dissolved in three hours, when I immediately killed the crow that had the tubes. Upon collecting and weighing all the flesh that remained in them, I found it to amount to about seven pennyweights. Hence in three hours and nine minutes it had lost four pennyweights.

On the other hand, the flesh tied to the string was reduced to half a pennyweight, which consisted of a packet of membranous or cellular fibres, the fleshy part having been entirely dissolved. This experiment clearly shews, that flesh left loose in the stomach is more speedily digested than when it is enclosed in tubes. And theory perfectly agrees with fact; for since solution is the effect of the gastric fluid, it is evident that the food, when loose in the stomach, is attacked by a larger quantity than when defended by the tubes.

LXIX. Young crows, as well as all other young birds, eat more than the adult; hence I suspected their digestion to be quicker. Having a nest of the grey species brought me in June, I made, among others, the experiment related in the last paragraph. The result was very satisfactory. A quarter of an ounce of beef, fastened as before, to a thread, had scarce touched the stomach, when the solution began, and in forty-three minutes was completed; but an equal quantity distributed in several tubes, required four hours and a half to be dissolved. Upon opening the stomachs of the two young birds, I immediately perceived the cause of this rapid solution; they contained half a spoonful of gastric

gastric fluid ; a quantity seldom met with in the stomach of adult crows. As the nestlings require more food, Nature has furnished them with the means of an easier and more speedy digestion.

It is scarce necessary to remark, that the experiments related in the LVth and following paragraphs, clearly evince this important truth, that the digestion of food is proportional to the quantity of gastric juice acting upon it. When this liquor comes in contact only with a few points, the decomposition is very slow and inconsiderable (LXVII); when freer access is allowed, the solution takes place more speedily, and is more considerable, (LXV, LXVI); it is very rapid, when every obstacle is removed, and the food is on all sides exposed to the action of the solvent liquor, (LXVIII, LXIX).

LXX. It is a question of ancient date, and still agitated by modern physiologists, whether certain carnivorous animals are capable of digesting bone. Among the various points, which I proposed to discuss in the present work, I conceived that this well deserved the reflection and attention of the philosopher ; I shall therefore both here, and in another part of my work, relate what I have observed on the subject. If we look at a crow and a bird of prey devouring an animal, we shall be disposed to think that the latter has the power of dissolving bone, but not the former. When, for instance, a hawk takes a pigeon, it first strips the back, and devours the muscular part of the breast ; then proceeds to the entrails ; and, lastly, swallows the ribs, vertebræ and head, not even sparing the feet and wings,

wings, if it should happen to be very hungry. When the same bird is given to a crow, it sets about stripping off the flesh; but when it has picked this clean, it leaves the skeleton. This rejection of the bones, is however very far from being an indubitable proof, in the estimation of the philosopher, that they are incapable of digesting them. At most it inclines us to believe it probable; but such probability requires to be confirmed by facts: and being engaged in enquiries of this nature, I could conveniently submit it to the test of experiment. As I happened to be provided with some phalanges of the human toes, I enclosed two in one of the usual tubes, which remained thirteen hours in the stomach. They weighed fifteen pennyweights at first; nor was this weight at all diminished, or the bones in the least softened. In doubt whether the too great thickness of these bones might not have prevented the gastric juice from acting upon them, I had recourse to smaller ones. Happening one day to find one of my crows dead in the apartment where I kept them, and the rest assembled round the carcase in crowds, and devouring it eagerly, I took one of the tibiae, broke it in two, and enclosed it in a tube. The tube continued a whole day in the stomach of another crow, but the bone was neither softened, or diminished in weight. The same thing was also observable, after the bone had been left loose in the stomach for fourteen hours longer.

LXXI. The greediness with which the crows devoured their companion, induces me to digress, for the sake of noticing a mistake
of

of the celebrated Dr. Cheyne. He pretends, that crows cannot digest the flesh of their own species; and that when they happen to swallow it, they vomit it up again. “*Ipse Cornix (says Haller, on the authority of Cheyne) cornicis canem ingestam non potest coquere & deglutitam vomitu rejicit (a).*” But the truth is, that the flesh which my crows devoured agreed very well with them, nor did they throw any of it up again. Further, in order to determine certainly whether the above-mentioned writer had fallen into an error or not, I killed and plucked another crow, and threw it into the chamber where its companions were kept, when they immediately leaped upon it, and devoured it with the same avidity as they had done the other, without afterwards vomiting the least particle of it. Upon killing and opening, three hours afterwards, one which appeared to have loaded its stomach more than any of the rest, I found the flesh partly dissolved, and in the form of a semifluid pulp, and partly in the process of solution, the very state in which I had seen other flesh.

LXXII. But let us return to the bones. It appears that, whether large or small, they are alike insoluble in the gastric juices of crows (LXX). But is this true likewise with respect to those of a soft structure, and which approach to the nature of a cartilage? This was precisely what I wished to know; and in order to determine it, I made use of another tibia, taken from an unfledged crow, and which therefore had not acquired its natural rigidity, though it was so hard as to break, when I tried to bend it: and now the gastric

(a) *Philosol.* T. 6.

liquor

liquor was not inactive. Of fifteen grains, which it weighed at first, it had lost five, when it had continued six hours in the stomach enclosed in a tube. It was become so soft, that it was capable of being bent into the shape of an arch. It continued to waste and become softer; and when it had remained twenty-seven hours in the stomach, it was so much reduced as to resemble a thin tube of paper. It was not at all gelatinous; and when it was pressed between the fore-finger and thumb, it shewed some elasticity, by recovering its former shape when the pressure was removed. It was not scabrous either internally or externally, but had rather acquired a greater degree of smoothness during its solution. In five hours longer concoction it lost the shape of a tube, and was totally reduced to pieces.

LXXIII. I tried other tender bones belonging to larger animals; and more or less of them was dissolved, but with difficulty, and after a very long interval. The solution was more speedy in young crows, probably on account of the greater abundance of their gastric juices (LXIX).

With respect then to the question concerning bones, we must conclude that they are indigestible by crows, except only such as, on account of their softness, are rather to be considered as cartilage than bone.

LXXIV. In the preceding, as well as the present dissertation, I have always spoken of the stomach as the place destined for the concoction of food. And in truth, whether we consult antient or modern physiologists, or consider my experiments, it will appear so clearly

clearly proved, that it would be absurd to entertain a doubt of it. But it may be proper to enquire whether this operation belongs exclusively to the stomach in the birds in question, or is partly carried on in the œsophagus. The foundation of this enquiry rests upon the manifest decomposition, which has been observed in that part of the food that is found in the œsophagus of some animals, as among others in the sea-crow and the pike (*a*). In order therefore to ascertain this point, I was led to make a few experiments, which I will relate after I have given a short description of the œsophagus and stomach in crows, and of the sources of the respective liquors in these two cavities.

LXXV. The œsophagus is membranous, and has no craw. When dilated it is cylindrical, if we except a slight contraction in the middle. To the naked eye it would seem destitute of follicular glands, which however become conspicuous when it is viewed with a glass. They are in such abundance, that there is not a single point of this canal without numbers of them. The excretory ducts are scarce discernible, though they emit the liquor of the follicles in great plenty. To see this, it is sufficient to pass the pulpy part of the finger over them. The liquor is of a viscid nature, of a cineritious white colour, and somewhat sweetish to the taste.

The inferior part of the œsophagus, has the same kind of fleshy fascia that has been noticed in birds with muscular stomachs. This

(*a*) Helvetius Mem. de l'Acad. 1719. Plot Nat. Hist. of Staffordshire.

fascia in crows is scarce an inch long; and in them too, as well as in the class of birds just mentioned, is a tissue of large follicular glands very evident to the naked eye, of a roundish figure, and full of a sweet fluid, less viscid than that in the small follicles in the membranous part of the œsophagus, but more dense, and of a lighter cineritious hue.

LXXVI. In the gallinaceous tribe we have spoken of three coats, the cartilaginous, nervous, and muscular (XLVIII, XLIX), which principally compose the stomach. These three coats are likewise found in birds with an intermediate stomach. When the cartilaginous is separated from the nervous coat, and the latter is viewed with the naked eye, it is seen to contain a multitude of whitish little bodies incased in it, which have the appearance of points; but when examined by the microscope, change their appearance to that of follicular glands, much smaller than those in the fleshy fascia (LXXV); these follicles are full of a viscid liquor; which they discharge at the extremity turned towards the stomach; when they are pressed by the finger, or any other body. The discovery of these glands in the nervous coat having afforded me room for imagining, that they might empty their contents into the stomach, I examined the cartilaginous coat with great attention, in order to try if I could find any minute pores for the transmission of the liquor into the cavity of that viscus; but I acknowledge ingenuously, that I could not discover any. This however by no means proves their non-existence; for they may be so small as to evade the sight, even when

aided by the microscope. And I cannot but believe, that these follicles, of which the excretory ducts are turned towards the stomach, are destined by nature for pouring their contents into that organ.

LXXVII. I now proceed to enquire whether, exclusively of the stomach, digestion is at all performed in the œsophagus of crows. In order to determine this, I firmly fixed to an iron wire two equal pieces of veal, one of them to the end of the wire, and the other two inches above. I then forced it down the throat of an hungry unfledged crow; the piece fastened to the end lay in the stomach, while the other occupied the œsophagus. To prevent them being thrown up, a string, fastened to the upper end of the wire, came out at the mouth, and was tied round the beak. Thus I was enabled to draw up the flesh at pleasure, and examine how much it was dissolved. In an hour the piece that lay in the stomach was quite consumed, except a little cellular substance; but the other piece was entire. It was again introduced into the œsophagus, and re-examined an hour afterwards; but now the œsophageal liquor had begun to act upon the flesh: its weight at first was six pennyweights, but now only five and an half. It was kept upon the whole six hours in the œsophagus, and lost nearly two pennyweights. These experiments will not permit me to refuse the œsophagus all power of digestion, an effect undoubtedly produced by the fluid of the follicles (LXXV); but it is inconsiderable when compared with that of the stomach, since this viscus dissolved six pennyweights of flesh in an hour,

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while the œsophagus dissolved but two in six hours.

LXXVIII. The experiments I afterwards made on young crows were still more decisive. The same wire was employed, and two bits of flesh were fastened to it just in the same manner, the one occupying the stomach, the other the œsophagus. The former piece was generally quite dissolved before the solution of the other began, though in time this also was very sensibly wasted. This diminution upon one occasion amounted to five pennyweights in the space of thirteen hours.

LXXIX. Lastly, in order to determine whether it was part only of the œsophagus or the whole of that canal in crows which possesses the power of solution, I formed a cylinder of flesh half an inch thick, and of the length of the œsophagus and stomach taken together. I fastened this cylinder longitudinally to the wire employed before, and forced it down the throat of a crow, so that one end touched the bottom of the stomach, and the other reached almost to the mouth. In a quarter of an hour the whole circumference of the cylinder was imbibed with a fluid, but at the lower end only, which rested upon the bottom of the stomach; the flesh had begun to be dissolved; here it was become whitish. In about an hour for near an inch, i. e. for the whole length of the stomach, scarce any of the cylinder was left; and what little remained was gelatinous and had lost its cohesion, while the portion that lay in the œsophagus appeared to be unchanged: but it did not continue so; a sort of erosion began to

take place along the cylinder, which went on, but with extreme slowness. And as this erosion extended along the whole length of the cylinder, I had reason to believe the whole length of the canal capable of concocting the food in a small degree, whenever it happened to be lodged there for several hours. But such an event never happens when crows take their food at pleasure, since the pieces never exceed the length of the stomach. In this respect they differ from some other animals, in which the food, after they have swallowed it, reaches into the œsophagus.

LXXX. Upon considering the great quantity of fluid continually dropping into the craw of gallinaceous fowls (LII), it appeared highly probable, that the concoction of the food is not a little promoted by the delay it makes there before it falls into the stomach. But the fact is just the reverse. The aliment is indeed softened and macerated (LII), but I could never perceive that it was at all dissolved: at least, I could never see any trace of it on several vegetable matters, which had been long retained in the craw. In a space more or less short they become soft, and are imbibed with a fluid; but I have not been able to perceive, that they were in the least dissolved. We must therefore conclude, that the œsophageal liquor in gallinaceous fowls is different from that of crows.

LXXXI. But why is the food so soon digested in the stomach, and so slowly in the œsophagus? Is it because the gastric fluid is more efficacious, or in greater quantity, than the œsophageal? What are the properties and characteristic marks of these two liquors?

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May we hope that experiments out of the body will be as instructive as those made in it? To enable myself to procure a large quantity of these fluids at pleasure, was the first step towards the solution of these problems. And as such a quantity could not be easily got by killing the birds, it became necessary to invent a contrivance for obtaining it from them alive. To put bits of dry sponge into the tubes, and leave them some time in the œsophagus and stomach, appeared to be the best means of attaining this end; for they must necessarily be saturated with the liquor of these cavities, and when vomited or drawn up, will supply the experimenter with a considerable quantity, provided he has used many tubes. Three tubes were introduced into the stomach of a crow, and four hours afterwards vomited up; the three little sponges, when they were taken out and pressed between the fingers, afforded thirty-seven grains of gastric liquor, which was frothy, of a turbid yellow colour, had an intermediate taste between bitter and salt, and being set to stand in a watch-glass, deposited in a few hours a copious sediment. As the sediment appeared to arise from the food that was dissolved by the gastric juice (for the bird had taken food a little before it swallowed the tubes) I repeated the experiment upon another crow, of which the stomach was empty, and continued so till the tubes were thrown up. This precaution I ever afterwards observed, at the same time taking care, that the fast did not last too long, lest it should induce a morbid state in the animal. I was likewise careful to cleanse the sponges from every impurity, by repeated washings

washings and dryings, before I made use of them again. Upon repeating, with these precautions, the foregoing experiment with the three tubes, I obtained thirty-three grains of gastric juice in a state of purity. It differed from the former in being of a transparent yellow colour, and in depositing very little sediment; it had the same bitter and salt taste. It appeared to have very little volatility, as it was kept several days in a watch-glass without suffering almost any diminution. When thrown upon burning coals, it extinguished them instead of taking fire; and when brought near a candle, it did not rise in flame. Further, paper soaked in it and thrown upon the fire, did not burn till the gastric fluid was evaporated. It had not more volatility or inflammability when just taken from the stomach and still warm.

LXXXII. The quantity, which was not inconsiderable, obtained from three sponges, gave me hopes of collecting enough for chemical experiments at large, and for attempting artificial digestion. Every crow was capable, as I found upon trial, of taking eight instead of three tubes; and as they would be thrown up in a few hours, I could repeat the experiment several times a day. Therefore to five crows, of which I then happened to be in possession, I gave forty tubes furnished with little sponges, i. e. eight to each. In three hours and a half all the tubes were returned by the mouth, and the quantity of gastric fluid expressed from them amounted to four hundred and eighty-one grains. Many days did not elapse before I had collected thirteen ounces of liquor. I employed it for the

the purposes for which I had designed it, and which shall be mentioned in their proper place.

LXXXIII. While I was engaged in these experiments, I observed several remarkable things. The first was, that the gastric juice flowed in great abundance into the cavity of the stomach; it sometimes happened, that one of the sponges was brought up in a quarter of an hour after it had been swallowed, and in this short space it was considerably loaded, and in an hour as much as it could be. Secondly, when a considerable quantity of fluid has been obtained, another, and even a third may be got immediately. Sometimes when a crow has vomited up the eight tubes, I put fresh sponges into them and returned them without delay; and this I repeated a third time and found, that the quantity procured the last time was as great as the second, and even the first. Thirdly, the fluid had always the qualities above-mentioned (LXXXI), if we except a difference of colour. It is commonly of a pale orange, but sometimes of a cineritious yellow.

LXXXIV. I took the same method to procure the œsophageal liquor, with the variation only of a single circumstance. The tubes were now fastened to threads, which were brought out at the mouth, and tied round the beak to prevent its being opened.

Thus the tubes were fixed in the œsophagus, without the least danger of their getting into the stomach, or being thrown up. Besides, I could draw them up at pleasure. I introduced four tubes at once into the œso-

phagus of a crow, and extracted them in three hours. I learned from this first trial, the scantiness of the œsophageal compared with the gastric fluid. The four sponges supplied me with eleven grains only. Doubting whether this might not be mere accident, I repeated the experiment several times, and allowed the tubes to remain longer in the œsophagus, but the sponges were very far from being so thoroughly saturated with fluid as in the stomach; so that direct experiment proves the great abundance of the gastric, in comparison with the œsophageal liquor. If the stomach and œsophagus of a crow be laid open longitudinally, the latter will be found to be moistened with its proper fluid only, while the former generally affords reception to part of it likewise. Theory too, in the present case, agrees with fact. The natural posture of crows, and indeed of most other birds, is such, that the liquor which oozes out from the internal surface of the œsophagus, cannot but descend to the lower parts from the law of gravity, and thence into the stomach. This organ must therefore be the receptacle of the œsophageal fluid; but it is more than probable, that it has a peculiar fluid also (LXXVI): besides, we are certain, that the bile is mixed in considerable quantity with the gastric juices. I have very frequently found the bottom of the stomach in crows full of it, and this is the reason why this juice is always bitter and yellow. Further, upon opening the duodenum longitudinally, I have perceived the yellowish green vestiges of the bile, which
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is discharged into that intestine at the distance of at least three inches from the pylorus through the cystic duct, which evidently arises from the gall-bladder. The conjunction of all these liquors, must produce a quantity of fluid far larger than that which derives its source from the œsophagus alone. And I doubt not but this is the reason, why the food is digested more speedily and perfectly in the stomach than the œsophagus (LXXVII, LXXVIII). Though I should also suppose, that this is in part owing to the greater energy of the gastric liquor from the admixture of the bile, which never rises into the œsophagus, as it appears from the juice of that canal never being at all yellow or bitter, but nearly insipid and colourless.

LXXXV. It remains now for me to speak of some attempts to produce artificial digestion with the gastric juices, reserving for another opportunity the recital of the chemical experiments made upon that fluid, obtained both from crows and other animals, with the view of acquiring as complete a knowledge as possible of its nature and properties. The greater abundance I was able to procure from crows, by means of vomiting, gave me the advantage of instituting a greater number of trials, than with that of gallinaceous fowls (LVI, LVII), from which the gastric liquor could not easily be procured without killing them. I first wished to examine the effect of the gastric juices of crows upon flesh, in the open air. It was January, and Reaumur's thermometer placed near the vessel used for the experiment, stood at the fourth
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and fifth degree (*a*). For greater certainty in these experiments, I established a term of comparison, by employing similar vessels containing the same flesh infused in water. I also took care upon the present as well as other occasions, that the flesh should be completely immersed in the respective liquors, and that the phials should be closed with stopples. For seven days the flesh kept in the gastric juice, and in water continued the same. On the eighth I perceived a slight solution, for upon agitating both liquors, several particles separated from the larger mass, and fell down to the bottom of the phials. No further progress was afterwards made, and the gastric fluid did not seem at all more efficacious than common water; only the flesh immersed in the former was preserved from putrefaction, but not in the latter.

LXXXVI. In this experiment I had used beef; I verified the same observation upon the more tender flesh of calves, chickens, and pigeons, notwithstanding the heat of the atmosphere had now raised the thermometer to seven degrees (*b*). While I was making these experiments in the natural temperature of the air, I was employed about others of a like nature in a warmer medium, viz. in a stove, in which the heat varied between (*c*) 22° and temperate. And now the effects pro-

(*a*) Wherever the thermometer is mentioned in this work, the same, viz. Reaumur's is to be understood.

N. B. The fourth and fifth degrees of Reaumur's thermometer answer to about forty-two and forty-three and one-fourth of Fahrenheit's.

(*b*) Forty-eight and three-fourths of Fahrenheit's.

(*c*) Seventy-nine and a half of Fahrenheit's.

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duced by the gastric fluid, differed from those produced by water. In the latter the flesh began to be a little dissolved in two days; this was an effect of incipient putrefaction, as appeared plainly from a foetid smell which began to be exhaled. The smell continued to increase during the following days, and in a week became intolerable, and the flesh was reduced to a nauseous pulp. In the gastric juice the solution was more rapid, and exhibited different phenomena; twenty-five hours were sufficient to decompose the flesh contained in it, and in a little more than two days there remained only a very small morsel entire. These solutions never emitted any bad smell; whence it is evident, that they did not arise from incipient putrefaction, like those in water, but from a more efficacious and a different menstruum, viz. the gastric liquor.

LXXXVII. Being now engaged by different occupations, I was obliged to interrupt these experiments, and could not resume them till the June following; and then taking advantage of the season, I exposed to the sun two phials filled to a certain height with gastric juice from crows, in one of which were immersed several pieces of beef, and in the other crumb of wheaten bread. Nine hours of sunshine much forwarded the artificial digestion, which was the object of enquiry. A good part of the flesh was reduced to a kind of glue, that when it was handled adhered to the fingers; nothing like flesh remained in any of the pieces, but the nucleus, which was still consistent and fibrous, and these two qualities it lost the next day; after having been exposed six hours to the sun the
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- nuclei, like the outside, no longer retaining a fibrous structure. The heat in the sun, as well on the first as the second day, was between forty and forty-five (*a*). The gastric liquor produced upon the bread a change analogous to that which the flesh had undergone. It not only lost its white colour and was turned grey, but had become viscous, and no longer presented to the eye the appearance, though it retained somewhat of the taste of bread. Of bread as well as flesh immersed in water and exposed to the sun for the same time, there was a perceptible diminution; but it was very superficial and inconsiderable when compared with that produced by the gastric fluid. The bread turned sour, and the flesh became putrid, circumstances that did not at all appear in the other phials.

LXXXVIII. Thus a tolerably complete concoction was obtained in the heat of the sun; but it was reasonable to suppose, that it would be still more perfect in the temperature of the stomach. In the preceding dissertation I have observed, that by way of substitute for the natural heat of the animal that furnished gastric liquor for the experiment, I fixed the tubes under my axilla (LVI, LVII); such an expedient was necessary, since glass is incapable of resisting the violent impulses of the gizzard. But there was now no longer the same danger, and the experiment might be made in the following manner. Several glass tubes six lines long and three in diameter, were hermetically sealed at

(*a*) An hundred twenty-two, and an hundred thirty-three and one-fourth of Fahrenheit's thermometer.

one end, and at the other bits of flesh were introduced, and then the tube was filled with gastric fluid. It was then very carefully stopped with sealing-wax, and the tubes were forced into the stomachs of several crows. Should digestion now take place it might be properly called artificial, since it must have been effected in close tubes, to which the juices of the stomach could have no access. But I soon found that the wax became soft in the animal heat, and consequently did not keep the tubes closely stopt, as I wished. There was however no difficulty in substituting a firmer cement, which would not be either melted or softened; and with such a cement I repeated the experiment just mentioned, and others of a like nature, which I will describe hereafter. I prepared two tubes in this manner; they were given to a crow, and returned by vomiting in an hour and an half. I will not conceal my amazement at finding, that the pieces of flesh enclosed in the tubes were not in the least changed, unless it was in having acquired a blueish red colour. My amazement was still more increased upon observing, that they had undergone no further alteration after remaining four hours longer in the stomach of the same crow, enclosed as before in two sealed tubes. These bits of flesh weighed in all twenty-eight grains; so inconsiderable a quantity would have been dissolved in a few minutes, if it had been loose in the stomach, and in a very few hours, if it had been enclosed in tubes open at the ends.

LXXXIX. Did this unexpected failure arise from the communication between the
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external air and that within the tubes being cut off, or from a deficiency of gastric fluid, or else for want of the action of the stomach upon the flesh? I considered maturely these conjectural explanations, but they appeared altogether insufficient. With respect to the last, it is repugnant to all those facts which prove the solution of aliment within tubes, open indeed at the ends, and perforated along the sides, but which effectually prevent the mechanical action of the stomach upon their contents. That the gastric fluid was in too small quantity for the solution of the flesh, is a suspicion unworthy of attention; for the pieces were always covered by it, so that the quantity of fluid must have been greater than that of solid. Lastly, the communication between the external air and that within the tubes being intercepted, cannot in all likelihood be the reason why solution did not take place. In order to determine this certainly, I made the following curious experiment. Having prepared several glass tubes of the length of six inches, I sealed them hermetically at one end, by means of a reverberated flame, and the opposite extremities were drawn out so as to form elongated cones. Through the open end of these cones I poured a quantity of gastric fluid, together with a few small pieces of flesh, which filled two thirds of the wider part of the cone. I then introduced the cones by their basis into the stomachs of some crows, allotting one to each bird; and when they rested upon the bottom of the stomach, their apexes came out at the mouth. To prevent their being thrown up, I used the precautions mentioned
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in another place (LXXVI). These conical tubes must no doubt have been very incommodious to the animals, but they were exceedingly well adapted to the end I had in view, since a free passage was allowed for the external air into them. However, notwithstanding this, the flesh remained several hours immersed in the gastric fluid, without shewing any sign of decomposition.

XC. It is proper to apprize the reader, that when the sealed tubes, or the cones, were kept long in the stomach, as, for instance, ten or twelve hours, the flesh was generally reduced to a dark-coloured gelatinous pulp. But this did not remove my surprize at seeing so slow a solution in those close receivers, in comparison with the rapidity of the process in the stomach. The gastric juice was quite fresh, it was in sufficient plenty, and the flesh put in the tubes and cones was exposed to the same degree of heat when it is in immediate contact with the sides of the stomach.

If crows are killed during the process of digestion, the bottom of the stomach generally abounds in gastric juice, which when compared with that expressed from the sponges, appears to differ a little, being more dense and bitter, and of a yellow inclining to azure. The juice which is mixed with the food, and occupies the upper parts of the stomach, approaches more to the nature of that with which the sponges are imbibed. Having learned from experiment, that digestion proceeds most rapidly at the bottom of the stomach, on account probably of the gastric juice being more active and efficacious there from

from its immediate mixture with the bile, which gives it the yellowish azure hue and a bitterer taste, I preferred this juice to that from the sponges, and repeated with it the experiments with the sealed and conical tubes mentioned in the LXXXVIIIth and LXXXIXth paragraphs. But the event did not answer my expectation, no solution of the flesh taking place till several hours had elapsed.

XCI. Upon comparing the laboratory destined by nature for the process of digestion, and these receivers prepared by art to accomplish the same end, I could discover but two differences; the flesh in the vessels undergoes the action of a fluid which is never renewed; while, on the contrary, in the natural laboratory it is continually subjected to the action of fresh juices, incessantly supplied by an innumerable multitude of follicular glands. Besides, the gastric juices being confined within the cavity of the stomach, there is little or no evaporation; whereas, when exposed to the air, and consequently cooled, they cannot but lose some of their more volatile and active particles by evaporation. Does then the slow solution of flesh in close tubes and in cones, depend upon the gastric juice being deprived by these two causes of part of that energy, on which digestion depends? I found from experiment, that the former cause, at least, the want of renovation had great influence in retarding the solution. If, instead of perfectly closing the tubes, I left a small perforation capable of allowing ingress and egress to the gastric juice, the solution of the flesh took place much

much sooner. The same thing happened, when, instead of leaving the same juice in the cones all the while, I was at the pains of changing it several times. But warmth is another condition absolutely indispensable for rendering the gastric fluid of these animals fit for digestion. When this liquor is kept in a temperature not more than four or five degrees above the freezing point, its solvent power is so much impaired, that it does not seem more efficacious than common water (LXXXV). This is also observable at seven degrees (*a*) (LXXXVI). In order to render the effects of the gastric juice perfectly sensible, a stronger heat is requisite, as from ten to twenty-two degrees (LXXXVI). Still solution proceeds very slowly; to remedy this the animal heat is necessary, viz. about thirty degrees (*b*) (XC). And so remarkable is the effect of heat in this particular, that the very liquor, which, for want of being renewed, concocts flesh slowly at thirty degrees (XC), effects this very speedily at forty and forty-five degrees (*c*) (LXXXVII).

XCII. Every time I expressed the juice from the sponges, I washed them in pure water, which was tinged yellow by the remains. After having made so many experiments on the gastric fluid in a state of purity, I conceived it might not be altogether without its use, to make one with the water in

(*a*) Forty-seven three-fourths of Fahrenheit's thermometer.

(*b*) Ninety-five one-half of the same.

(*c*) One hundred and twenty-two, and one hundred and thirty-three one-fourth of Fahrenheit's thermometer.

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which the sponges had been washed; with this water I therefore filled a small glass phial, which was left exposed to the sun, with a piece of flesh in it, for three days in July. The flesh (which was mutton) shewed some signs of solution. On the third day, there appeared upon the bottom of the phial a quantity of impalpable matter of a cineritious colour, consisting of particles separated from the flesh immersed in the liquor. Notwithstanding the season, as it usually is in July, was very hot, it had acquired little or no foetid smell; whilst a similar piece of flesh, exposed to the sun in the same manner, but immersed in water, became intolerably putrid on the second day.

XCIII. But it is time to quit the subject of digestion in crows, and to proceed to that of herons, the other species of bird which I proposed to examine in this dissertation. The herons upon which my observations were made, and which the nomenclators denominate *cineritious*, or *grey* (*a*), must certainly be classed among birds with intermediate stomachs, since the sides of this viscous have an intermediate thickness and solidity between membranous and muscular stomachs. When this organ is dilated, it appears about two inches wide, and as many long; its form approaches to that of a cylinder. When opened lengthwise, and observed internally, it presents the appearance of rugæ, of which some run in a longitudinal, some in a transverse, and others in an irregular and oblique direction. The sides of the stomach are co-

(a) Linn. Syst. Nat. T. I. Bel. Av.

vered with a kind of gelatinous lining, of some consistence, but easily removed, and of a colour between white and yellow. This lining seems organized; and I should be inclined to suppose, that it is the innermost coat of the stomach. The nervous coat next presents itself; it is of a whitish colour, and moderate thickness, but its texture is strong, and it is not easily lacerated. When this coat has been cleaned and dried with a napkin, and then distended, and compressed underneath, it will be immediately covered with an effusion of very small and scarce visible drops, which enlarging and approaching towards each other, form at last a thin aqueous veil. And if this veil be wiped away, and the nervous coat be again distended and squeezed, another like the first will appear; and in like manner a third, a fourth, &c. with this difference only, that the quantity of humidity is every time diminished. There can be no doubt of this being a portion of the gastric fluid, discharged directly into the cavity of the stomach. I have employed the utmost attention in searching whether this liquor derives its origin from glands, or any analogous bodies, but could never discover either the one or the other; and therefore it must be supposed to be secreted by small arteries, which open into the stomach, and deposit their contents there. After the nervous, we have the muscular coat, of a red colour, and scarce a line in thickness. It is composed by fleshy striæ, partly transverse, partly longitudinal. The former appeared to me to occupy the surface only, the latter constitute the internal strata, and are continued to the termination

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mination of this coat. There is also another coat, consisting of cellular substance, and this is the last of all.

XCIV. The stomach always, and especially when empty, contains more or less gastric fluid, of a bitter taste, turbid yellow colour, and generally of some density. The bitterness is owing to the bile, which has this taste, but in an intenser degree; I have often found it at the bottom of the stomach, and in the vicinity of the pylorus. The gall-bladder in length exceeds an inch; its greatest diameter is of five or six lines; in shape it resembles a small egg, of which the sharp end is inserted into the liver. Notwithstanding many careful examinations, I am not certain of having found the cystic duct; I however suspect that it perforates the duodenum, at the distance of six inches from the pylorus; this I collect from a line of an azure-yellow hue, which arises from the gall-bladder, and is inserted into that part of the intestine.

XCV. Above the stomach we meet with the same kind of fleshy fascia, which I have noticed in gallinaceous fowls and crows, (LXVI, XLVII, LXXV). In the grey heron it exceeds an inch in breadth. This fascia is also covered with the same gelatinous lining that invests the stomach (XCIII). Next we find the nervous coat of a finer texture than that of the stomach, of which it appears to be a continuation. This coat, when attentively viewed, looks like a sieve, so much is it perforated at every part. The perforations are nothing but the apertures or mouths of subjacent follicular glands, occupying almost all the inside of the fascia, and
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visible through it. If the nervous coat be any where compressed, a viscid, cloudy, and, as far as I could judge, insipid liquor oozes out at these mouths, and continues to ooze out if the pressure be continued. The follicles that lie beneath, manifestly supply this fluid in such abundance. It would be superfluous to describe these glandular bodies, since they exactly resemble those of crows and gallinaceous fowls; whether we consider the immense number of them, or their position, contiguity, shape, or colour. When we raise this aggregation of follicles, we come to the muscular coat, which is very thin, and consists of several strata of long and compact fleshy fasciculi; next to which lies the last or external coat, the thinnest of all, and consisting of cellular substance.

XCVI. The œsophagus is about twelve inches in length, and in breadth one and a half. Its shape is nearly cylindrical, but it becomes narrower near the stomach. Upon examining it externally with a microscope, I discovered that it was quite full of minute bodies, which are, I suppose, glandular. When it has been carefully inverted and inflated, and the humidity, which always covers it, has been wiped away, if now we lay hold of one end, and squeeze it forcibly, so that it shall be enlarged in the adjacent parts, the humidity will appear again; and upon repeating the compression, it will be seen several successive times, just as in the stomach, (XCIII); with this difference, however, as I conceive, that the humidity in the stomach derives its source from small arteries, and in the

œsophagus from minute glands, or some analogous bodies.

XCVII. It was natural to suppose that this apparatus of fluids, which are constantly trickling into the cavity of the stomach and œsophagus of herons, are chiefly designed for digestion. But the small number which I possessed, and their almost never vomiting, like crows, indigestible bodies, and consequently tubes, did not permit me to make such a series of experiments as I could have wished. I have however been able to make the most essential, of which one consisted in enquiring into the manner of digestion in the stomachs of these birds. For this purpose I had recourse to the tubes, than which I do not believe there are any means better adapted to such enquiries. It is well known that the grey heron feeds on fishes, frogs, water-snakes, and several sorts of aquatic worms and insects. Those in my possession devoured frogs, and especially fishes, with great greediness; and I therefore used them for my experiments. They swallow frogs of a moderate size whole. A whole frog, inclosed in a tin tube, was introduced into the stomach, together with a fish, of bulk nearly equal, inclosed in another tube. In twenty-four hours the heron was killed, and the stomach opened; though the tubes were very thin, they had received no damage, if we except two slight contusions upon one of them; they were so light, that it was not difficult to guess that they no longer contained the same quantity of matter that had been put into them. The little fish was all dissolved, except some of the ribs, a few bones of the head,

head, and a bit of the flesh of the back, which was so tender, that the parts no longer cohered. The frog's shape was more easily distinguishable than that of the fish; the pulp of the thighs, and even the bone itself, was quite destroyed; but the ends of the lower, as well as the upper limbs remained. The integuments of the abdomen and thorax were no longer to be found; and the subjacent flesh was become so soft, that it appeared to have undergone a slight boiling. The bones had acquired the softness of cartilage. These remains of the frog and fish were impregnated with gastric fluid, and tasted bitter. The intelligent reader perceives the immediate consequences of this experiment. In the first place the stomach of herons acts with some force upon the substances it contains, as we may collect from the slight contusions upon one of the tubes. Secondly, digestion, which was in an advanced state in the frog, and quite complete in the fish, is not the effect of trituration, but of the gastric fluid, which entering in at the open extremity of the tube, or through the lateral holes, penetrated the two animals, and by virtue of its solvent power, partly consumed them; but it made greater havock upon the fish than the frog, on account of its being tenderer. Thirdly, the efficacy of the gastric fluid of the heron is not limited to the solution of soft parts, such as the skin, flesh, &c. but extends to the hardest also, viz. the bones.

XCVIII. Of the last circumstance I wished to obtain greater certainty, by putting bones alone into the tubes. We have already seen that crows are incapable of di-

gesting hard bones, and that they digest such as are tender with difficulty (LXX, LXXII, LXXIII). It was therefore an object of great curiosity to discover what would happen in herons; and it was easy to satisfy this curiosity, by inclosing bones of several sorts in the tubes; in one I therefore enclosed only the tender bones of frogs or fishes; in another hard bones, viz. the thigh-bone of a turkey broken into two pieces. The pieces of both the hard and tender bones were formed into two bundles, and tied with thread. After an heron had retained these two tubes in its stomach twenty-seven hours, it was killed. It was with a mixture of surprize and pleasure that I saw the tube which contained the fishes and frog's bones empty, while the string remained entire. The gastric juice had then perfectly dissolved the bones. But this was not the case with respect to the contents of the other tube. I should have considered them as untouched, if they had not appeared smother, whiter, and perhaps thinner than at first. They now weighed only eleven pennyweights and six grains, whereas at first they had weighed fourteen pennyweights; they had therefore lost of their original weight three pennyweights within six grains. If this experiment be compared with that made upon crows, with the same intention, it will appear evident, that the gastric juice of the latter is less efficacious in dissolving bones than that of the heron. And in truth, their nature requires that they should digest every part of the animals upon which they feed. I gave them some frogs, and
watched

watched their way of eating them; when of a moderate size they swallow them whole, when very large they separate them into several portions, and swallow them without parting the flesh from the bones. Since, then, herons do not enjoy the advantage of vomiting up substances incapable of being digested (LXXVII), and the bones of frogs and such animals cannot easily be voided at the anus, Nature has wisely endowed the stomach with the power of concocting and assimilating bone.

XCIX. It was equally curious and important to enquire, whether the œsophagus of the heron as well as of crows, is capable of performing digestion (LXXVII, LXXVIII, LXXIX). The great length of the neck, and consequently of the œsophagus, is extremely favourable to such an enquiry. A flead frog was forced half way down this canal with the head downwards, where it was fixed by a string, of which one end was tied round the hind legs, and the other came out of the mouth, and was wrapped round the neck. In this situation it remained two hours, and more was effected than I could expect in so short a space. The animal was indeed entire, but was become very tender, though the tenderness did not penetrate far below the surface. This appearance of incipient concoction induced me to push the experiment further, that I might see how it would terminate. The frog was therefore replaced in its former situation, where it continued three hours longer; I then thought it time to examine the animal, and drew up the packthread, but nothing came up with it except
the

the hind legs and ribs; the rest remained in the throat, and an instant afterwards I perceived the animal transmit it into the stomach. I found the legs and thighs half dissolved, and being very desirous of knowing what had happened to the other parts of the animal, I determined to kill the heron without delay. The frog was in the stomach; the external muscular flesh was quite destroyed, and what remained entire was easily divisible into several portions, especially at the articulations. The same appearance of decomposition had taken place, as if it had been macerated in water; but it did not afford the smallest mark of putrefaction.

C. Although experiment thus abundantly evinced a sensible concoction in the œsophagus, I had not adverted to one circumstance, which however deserved attention, viz. to fix the precise loss which the flesh underwent in that cavity. I therefore repeated the experiment with this view, but having no frogs in my possession, I substituted some flesh with which I happened to be provided, consisting of cow's lungs, to the amount of half an ounce, forty grains. It was drawn out of the œsophagus by means of the packthread, after it had remained there thirteen hours. It had lost seven pennyweights and two grains.

As the œsophagus of the heron is membranous, it is more than probable, that its mechanical action did not concur in producing this effect; it however was proper to prove this by direct experiment, which might be done by means of the tubes. With them therefore I repeated the experiment which I

had before made, in order to determine, whether the œsophagus of the heron is capable of digesting food. Solution of the flesh undoubtedly took place within the tubes, and I was convinced, that it did not depend on any motion of the œsophagus, but on the efficacy alone of the fluid which is secreted by it.

CI. Another experiment yet remained to be made, whence we might deduce not only the exact diminution of the flesh, but the proportion between its diminution in the œsophagus and stomach; two globular pieces, two-thirds of an ounce each, of cow's lights were introduced, one into the œsophagus, and the other the stomach; each remained seven hours in its respective situation, when the heron was killed; the ball from the stomach at first of the size of a walnut, was now no larger than a pea, and weighed only twenty-eight grains. That which had lain in the œsophagus was indeed reduced in bulk, though but very little in comparison with the other; it had lost three pennyweights eighteen grains.

Both these instances gave me an opportunity of remarking, that the juices, whether of the œsophagus or stomach, did not seem to act by penetrating deeply into the substance of the flesh, but by corroding the surface; the external layer was first dissolved, and then those that lay beneath in their order. And in reality, when I came to wash the ball taken from the œsophagus, and wipe away the external gelatinous stratum already dissolved by the œsophageal fluid, the next stratum shewed the natural fibrous, firm, and red appearance; and when the ball was cut
into

into two hemispheres, the inside seemed perfectly sound, without the smallest sign of having been impregnated or touched by that corrosive liquor. The same observation is applicable to the other ball; for notwithstanding its great diminution, within it was quite sound.

I had now but two herons left, and these I sacrificed to the desire of ascertaining still further the excessively rapid concoction of the stomach, compared with that of the œsophagus. And in fact, I observed it again on two frogs and as many fishes, of which the former continued eight hours in the œsophagus and stomach, and the latter nine hours.

These experiments incontrovertibly prove, that the œsophagus of the heron as well as of the crow, has the privilege of digesting any food that may happen to be lodged in it: this privilege extends likewise to other animals, as we shall see in some passages of the following dissertations.

CII. The observations related in the present and the preceding dissertation, present us with various instances of agreement and disagreement in the digestion of birds with muscular and with intermediate stomachs. Let us here, for the convenience of the reader, collect into one point of view these scattered traits; they may fix more firmly in the mind all that we have observed, whether curious or interesting, in these two classes of animals. With respect to the traits of resemblance, they may all be reduced to the relation between the gastric fluids. First then it has been proved, that these fluids,
besides

besides being alike in colour, are always salt and bitter; and that the bitter taste derives its origin from the bile, which regurgitates through the pylorus into the cavity of the stomach. Secondly, these fluids are the immediate agents of digestion, both in muscular and intermediate stomachs, independently of trituration. Thirdly, In these two orders of birds the fluids act in the same manner in the solution of the food; they first soften and next convert the surface into jelly, then produce the same effect upon the interior parts, and so insinuating themselves gradually till it is completely dissolved. Fourthly, they do not entirely lose their solvent efficacy as soon as they are taken out of the stomach, provided they are heated to a proper degree, as artificial digestion proves. Lastly, The fountains from which these fluids spring are, in great measure, the same in both classes, viz. the follicular glands, with which their organs abound.

CIII. The differences are in part reducible to the inferior efficacy of the gastric fluid in muscular to that of the same fluid in intermediate stomachs. Thus the gastric fluid in the former is incapable of dissolving the same aliment, which in the latter it easily dissolves. In like manner the food, which each kind of gastric juice decomposes and digests, is sooner subject to this change from that which belongs to intermediate stomachs. And this is also the reason, why artificial digestion succeeds much sooner with the first than the second. The same inefficacy that the gastric juices of birds with muscular stomachs shew in decomposing certain aliments of a firm texture,

texture, extends also to their œsophageal juices in decomposing soft substances, notwithstanding the latter are tolerably well decomposed by the œsophageal juice of birds with intermediate stomachs. The prodigious effects of trituration in muscular stomachs, constitute another very striking difference between these two classes of birds, the feeble force of intermediate stomachs being scarce comparable with the enormous power of the other kind. Such a degree of force was absolutely necessary in these, since the juices are incapable of decomposing food of considerable firmness, such as seeds, the natural food of birds with gizzards; and therefore an agent capable of breaking, triturating, and thus pre-disposing them for digestion became necessary; and such are in reality the gastric muscles in these fowls.

DISSERTATION III.

CONCERNING DIGESTION IN ANIMALS WITH MEMBRANOUS STOMACHS. FROGS. NEWTS. EARTH AND WATER-SNAKES. VIPERS. FISHES. SHEEP. THE OX. THE HORSE.

TO examine at full length the nature of digestion is the object of these dissertations. By extending my enquiries to the three classes to which all animated beings may be referred, I hope to be enabled to solve the problem in a satisfactory manner. Of these classes the first comprehends animals with muscular, the second with intermediate, and the third with membranous stomachs. The last class is infinitely more numerous than the two former. If we suffer our imagination to range over the immense multitudes of quadrupeds, fish, reptiles, birds of prey, not even excluding man himself, we shall find, that they are all, or nearly all, endowed with membranous stomachs; not to mention that numberless tribe of minute beings, the greater part of insects. My task
would

would have been endless if I had projected enquiries, I will not say concerning every species of animals included under these genera, a project which many academies would not be able to execute, much less a single observer; but concerning great part of them. I was therefore obliged to confine myself to researches upon a small number. These researches combined with others already related in the two first dissertations, will be sufficient, if I am not very much mistaken, to set the theory of digestion in a clear point of view, both in animals and man. As the various species, which I take into consideration, cannot be all exhausted in a single dissertation, I shall distribute them into several, beginning with the animals that are situated lowest in the scale of sentient beings, and ending with that which occupies the highest and noblest place, with man.

CV. Let us begin then with frogs and water-newts, two species of small carnivorous animals. As the mouth and œsophagus in the former are large, it was easy to introduce tubes into their long stomachs. But I was soon aware, that it would be necessary to make experiments upon a great number at once, if I wished to know what changes the flesh enclosed within the tubes underwent in the course of several days; for the tubes were very often vomited up, and at uncertain intervals, sometimes in a few, sometimes in several hours after they had been swallowed; at others again, after a whole day, and in some instances, after a still longer interval. As I knew, that this species of animal very greedily devours any sort of flesh that falls in its way,

way, I did not think of selecting, but took what happened to come first to my hands, and this proved to be a piece of the small intestine of a sheep, which I divided into twelve portions, and enclosed them in as many tubes. These tubes were distributed among six of my largest frogs, two being allotted to each. They were kept in a very large vessel of water with high perpendicular sides, that they might not make their escape. I neglected the tubes that were thrown up, and examined those only which remained in the stomach. In the space of a day I observed the following results. From the intervals of the grating which lay before the open extremities of the tubes, there oozed out a cineritious matter, which, when touched, adhered to the fingers, and formed long filaments. When the grating was removed, I perceived, that this gluten was nothing but the flesh itself, which at that part began to be decomposed and to change its nature, retaining however the characteristic marks of flesh in the more internal parts of the tubes. Upon opening the stomachs I did not find any gastric fluid; they were quite dry.

CVI. In two tubes that were examined at the expiration of two days, the flesh had undergone a further decomposition. It now not only oozed out at the meshes of the lattice-work, but likewise at most of the perforations in the sides of the tubes; and when it was drawn out with the point of a pair of forceps, and then freed by washing from the viscid mucilage, what remained of real flesh or intestine was so very little, that I do not believe it exceeded the thirtieth part of its original

ginal weight. At the end of the third day there remained but a single tube in one frog; in this there was no flesh, but it had been all dissolved into gluten, had oozed out of the tube, and adhered to the sides of the stomach, excepting a very small portion that was sticking to the tube. This viscid matter was insipid to the taste, a certain proof that the gastric fluid had effected this alteration without the concurrence of any mechanical action of the stomach. It must however be allowed, that this fluid is exceedingly slow in producing its effects, since it required three days. This slowness must have arisen either from the small quantity or inefficacy of the fluid, or perhaps from both causes. In consequence of this tardy action I found in similar experiments upon six other frogs, that the flesh in some of the tubes was not entirely consumed at the end of the fifth day.

CVII. The gastric liquor of frogs is not however on this account incapable of concocting in time substances which we should have supposed above its power, viz. bones. In a quantity of frogs brought me one day by the fishermen, there was one so large, that I was induced by its enormous size to kill it, in order to see what it contained; I found, that the enlarged bulk was owing to a mouse in the cavity of the stomach. The hair had begun to fall off, and the skin was become so very tender, that it had lost its cohesion. The fore as well as the hind legs had undergone a greater degree of solution, the naked bones only being left, and they were considerably wasted and converted into a semi-gelatinous substance. The mouse upon being opened

appeared quite sound internally, the destruction was entirely confined to the surface, and therefore occasioned by the gastric fluid, which had begun to act here on the external parts, just as it does in animals with muscular and intermediate stomachs. The thinness of the extremities permitted the fluid to penetrate them with greater facility, hence it had almost consumed them without sparing even the bones. In this instance I could not perceive any sign of trituration, for the mouse was neither bruised nor lacerated; nor can I conceive what other force can be exerted by stomachs composed of such fine coats, besides that of compressing the bodies they contain, when they happen to be very large.

CVIII. The mouth and throat of water-newts are both so narrow, that they would not admit the usual tubes, they however admitted others made in the same form, but of a smaller size, on purpose for them. From having kept these animals in my house for several years, both when I had occasion to examine the circulation of the blood, and to observe the admirable reproduction of their limbs, I had learned, that the food which they devour with the greatest avidity is living earth-worms (*a*). Nearly the same observation is made by my illustrious friend Mr,

(*a*) I treat at full length of these aquatic lizards in my three works intituled,

Prodromo di un' opera da imprimerfi sopra le riproduzioni animali,

Del Azione del Cuore ne' vasi sanguinei,

De' Fenomeni della Circolazione osservata nel giro universale de' vasi.

Bonnet, in his fine memoir *concerning the reproduction of the limbs in water-newts*, in which he confirms my discovery of this wonderful reproduction in the clearest and most decisive manner, after it had been questioned by Messrs. Adanson and Bomare, for want of address and skill in making experiments on this branch of zoology (*a*). I had then recourse to earth-worms; they were cut in pieces, and placed alive in the tubes, which were introduced into the stomachs of several salamanders. The gastric fluid of these little reptiles acted more speedily than that of frogs (CVI). The divided worms began to change colour in fifteen hours, and to become soft and flabby. About the thirtieth hour the parts had lost their cohesion, and the rings were no longer visible; and in less than two days they were converted into a whitish pulp, of which the greatest part had run out of the tubes.

CIX. The dissection and examination of the stomachs of newts, presented me with a phenomenon, which must not be concealed from the reader, both on account of its singularity, and the light it throws on the present subject. This phenomenon is nothing less than a great number of small white worms in this viscus, visible to the naked eye; of the thickness of a thread, and the length (at least the largest) of two-thirds of an inch; however if we wish to examine them minutely, we

(*a*) The memoir is inserted in Rozier's Journal for November, 1777.

N. B. It is likewise reprinted in the late collection of his works.

must employ the microscope. They are of two sorts; in one both extremities terminate in a point, the other has one end pointed; but the other obtuse, and marked with a dark spot; the latter species is shorter than the former, and thinner in the same proportion. Each species is furnished with rings, narrower at the ends of the body, and wider at the middle, as is generally the case in annular worms. These two sorts of worms are not flat or compressed, but round; it is therefore certain that they do not belong to the genus of *tænia*, or the *gourd-worm*, but to that of round or columnar worms (*teretes*). They are not loose in the cavity of the stomach, as the worms lodged in the intestines of larger animals commonly are, but are always found with one extremity inserted to some depth in the internal coat of that organ; hence it requires some force to detach them, and frequently they break sooner than separate. Of those that have the dark spot, the more obtuse end is fixed in the stomach; it is impossible to say whether this is the case with respect to others, since both ends are equally pointed. The loose extremity projects into the cavity of the stomach, sometimes coiled up in the form of a circle, and at others twisted in a spiral. If the stomach be taken out of the animal, and set to macerate in water, the worms live for many hours without quitting their situation: if afterwards we separate them with the hand, without breaking them, and place them upon some substance, in order to observe their movements, they will be seen to writhe in various directions; sometimes bringing the

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mouth

mouth towards the tail; sometimes stretching themselves in a right line; and at others making strange contortions, as is usual with reptiles in general.

CX. Not being able to conjecture for what purpose the part perpetually inserted in the substance of the viscus, could be designed, unless it was to suck the thinnest and purest part of the liquor; and consequently supposing it to be the head of the animal, or at least some analogous part, I tried to discover the mouth with the aid of the microscope; but my endeavours were vain. I believe, however, that I found the alimentary canal; it is a bright silver-coloured species of intestine, running along the worm, in a tortuous manner, from side to side; it is always full of a number of particles, which fluctuate regularly, like a buoy, probably impelled by a sort of peristaltic and antiperistaltic motion. This canal is common to each species; in that with a dark spot at one extremity (CIX), a second canal may be perceived; it is strait, and probably (I should rather say certainly) the receptacle for the eggs; for I have always observed it more or less full of a great number of corpuscles, of an oval shape, floating in a very transparent lymph; these corpuscles, when the worm is not in motion, always continue at rest. If we lay hold of the animal by its extremities, and break it in the middle, the little canal will generally be broken, and the ovula will make their escape in a stream from the lacerated part. It is not difficult to burst them between two pieces of talc, when a thin fluid spirits from them; after which the eggs be-
come

come dry and opaque, consisting now of nothing but the empty envelope, as always happens to the membranaceous eggs of small animals. Each worm of this species is furnished with those oval particles enclosed in their canal; if they are real eggs, as there is great reason for supposing, we must conclude that every individual is an hermaphrodite; it will however remain doubtful whether they are strictly so; i. e. have no need of copulation, like sweet water polypes, and many other sorts of microscopical animals, &c. or else, in the wider acceptation of the term, are like testaceous and naked snails and earth-worms; each of which brings forth eggs and living young, but requires the concurrence of another individual.

CXI. It would not be an improper question, if I were asked whether these worms lodge in healthy newts, or rather in such only as are diseased. This doubt suggested itself to me; and in order to clear it up, I examined not only such as I kept at home, and were therefore liable to the suspicion of unhealthiness, but such also as were newly caught, and full of health and vigour; but the stomachs both of the one and the other harboured alike these unpleasant guests. But it must be observed that they do not fix their abode in all newts; and that in those where they do, they are not equally numerous. Of the immense number I have opened at different times, and with different views, three-fourths have had a family of worms in their stomachs; which is sometimes composed of only five or six individuals; at

others of several dozens, and at others again of an hundred or more.

CXII. In my numerous examinations of the stomachs of the different animals mentioned in this work, crows alone have exhibited a phænomenon nearly resembling what is found in newts; I mean a quantity of worms lodged in the stomach. But these worms are not inserted into the internal coat, as in newts, but are found between the internal and the nervous. We are very well acquainted with the little worms that live in trees, and generally fix their abode between the bark and the wood; and lurking there unseen, devour the cortical part, which furnishes them with an agreeable aliment. If the bark should be parted from the trunk on purpose, or by accident, their devastations are exposed to view, in the form of excavated paths, winding backwards and forwards in a serpentine direction; nor is it uncommon to surprize the worms actually employed in forming these excavations, which serve them at once for food and lodging. The same thing nearly is observable with respect to the worms of crows. If the internal be parted from the nervous coat slowly and carefully, these animals are suddenly exposed to the eye, adhering for the most part to the back of the internal coat, lurking in certain cavities formed in its substance, and which in all likelihood, arise from the erosion of these very worms. Further, we find some with both ends exposed, while the middle is deeply buried in the substance of the internal coat. Lastly, others have one extremity inserted into this, and the other into the adjacent

cent nervous coat ; but they never make their way into the cavity of the stomach. These worms do not appear to differ from those in newts, in colour, length, thickness, or in the alimentary canal ; they have however one essential difference, they are without rings, but have a smooth and slippery skin. In their motions they are dull and languid ; when taken from their abode, and placed in water, they live many hours. They are found both in grey crows and rooks ; but I have never seen them in any part of the body except the stomach.

CXIII. But let us return to the worms in newts (CIX, CX, CXI), and consider them in as far as they relate to digestion. I assert that their presence is an incontrovertible proof, that no sensible degree of force is exerted by the stomach ; for how is it possible to conceive that the sides of the stomach can rub against each other, or at least impinge against the food, without doing the smallest injury to the delicate structure of these worms ? I have more than once taken the stomach of a newt between my thumb and finger, and compressed it very gently, or rubbed it lightly, and upon opening it, have always found some rupture, some discontinuation in the parts of these worms. We must therefore conclude, that digestion in water-newts is solely the effect of the gastric fluid, of which the efficacy has been already shewn in the decomposition of earth-worms inclosed in tubes (CVIII). I have also seen this efficacy, in a manner equally striking, on worms which newts have taken and swallowed of their own accord. How tenacious these minute reptiles are of life, is abun-

abundantly proved by cutting them into several pieces, in consequence of which they do not die; but on the contrary multiply, as many worms being produced as parts into which they were divided (*a*). It is true, they do not cease to live after having remained ten or twelve hours in the stomach of a newt; nay, when they fill it too full, they void some alive and crawling by the mouth, whether by actual vomiting, or whether the worms, after various movements in so disagreeable a place of confinement, at last find their way out through the œsophagus. But they certainly die at last, not because they are triturated or crushed to pieces, for they continue whole several hours; the gastric fluid first softens and then converts them into a gelatinous substance, and by a continuance of its action, at length reduces them to an impalpable mass.

CXIV. But how came the tender worms in the stomach to escape solution, when all other insects, whether aquatic or terrestrial, upon which the newt feeds, die and are digested? If it should be said, that this happens because they have been habituated to the stomach by long residence, the difficulty would be perhaps removed to a greater distance, but certainly not taken away altogether. As the cause of this phenomenon, we must assign the inability of the gastric fluid to decompose these minute beings, however powerful may be its energy upon others of a structure less delicate; just as a chemical

(*a*) See Reaumur, Bonnet, and my Prospectus.

menstruum is capable of dissolving one metal, but not another. Thus aqua regia dissolves gold, but not silver; or an acid that combines with the calcareous, has no attraction for the argillaceous and siliceous earths. Nearly the same difference of digestion is also observed in that of polypes provided with arms; they sometimes swallow their own arms along with insects; but though the former die and are digested, the second do not in the least suffer. Thus a polype inserted into the stomach of another polype, continues to live as before (*a*).

CXV. But let us proceed to serpents, of which I proposed to treat after frogs and newts. Those which are most easily procured in the environs of Pavia are certain terrestrial snakes, called in some provinces of Italy, *Smiroldi* (*b*); water-snakes, which many naturalists call swimming (natrices) (*c*). The first considerably exceed the natrices and vipers in size. The largest are about an inch and a half in thickness towards the middle of the body, and forty-five and sometimes fifty inches long. The lower part of the body is white mixed with yellow and green streaks, the upper part is blackish, but towards the neck and head interspersed with a milky white. They fly with greater speed than the water-snakes, and far greater than

(*a*) Trembley, Mem. sur les polypes.

(*b*) Not described by Linnæus or any other naturalist, as far as I know.

(*c*) *Natrix*. Linn. Syst. Nat. T. I. *Natrix torquata*. Ray. quadr.

vipers. They are not inferior to the latter in a spirit of revenge, and their bite also draws blood, as I have myself experienced, but is harmless. Before I made use of the tubes, I wished to acquire some knowledge of the œsophagus and stomach. Having therefore skinned one, and blown up the œsophagus in such a manner that the air could neither pass out above nor at the pylorus, it appeared to me to resemble a large intestine, cylindrical for about the length of nine inches, and becoming gradually narrower below, so as to form a funnel of the length of four inches and an half. I soon perceived, that this funnel was the true stomach, and the intestine the œsophagus. Both the trachea and lungs run along the œsophagus, to which they are firmly attached by means of a membrane, as also is the heart, which has the shape of an elongated pyramid, situated at the origin of the lungs. We find likewise a viscus arising from the basis of the heart, ascending upwards along the œsophagus, and adhering in great measure to the trachea: it is of the same length as the lungs, but its substance is different, being tender and ash-coloured; I could not then determine what it was. Next below the lungs lies the liver, which, together with the *vena portarum*, resembles a long narrow leaf attached to a very long footstalk; both adhere to the œsophagus. Below the stomach we find the spleen, nine lines in length and of a very acute oval shape. The gall-bladder lies in the region of the small intestines, consequently at a great distance from the liver; when we press it the duct is filled with bile, which
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it evidently discharges into the duodenum at about the distance of an inch from the pylorus. Near the gall-bladder we find another body smaller than it, attached to the duodenum, and of a fleshy consistence. I should suppose it to be the pancreas.

CXVI. If we separate the œsophagus and stomach from the lungs and other parts just described, and open it longitudinally, the œsophagus appears simply membranous; the membrane of which it consists is very thin and of a silver colour. The stomach is composed of thicker sides, and among the coats which compose it we have one of flesh, which like the fleshy coats of other membranous stomachs, is very thin. I could not perceive, that the œsophagus is provided with any glands or follicles; but I observed, that the stomach was abundantly supplied with them throughout its whole length; they discharge part of their liquor on being pressed, and the internal coat is moistened with it.

CXVII. I come now to experiments relative to digestion. I found great facility, not only in passing the tubes into the stomach, but likewise in bringing them up whenever I pleased. I made an assistant lay fast hold of the snake so as to prevent its striking or wreathing round the body, while I opened the mouth and forced a tube in lengthwise, and then, by means of a thin rod, thrust it two or three inches down the throat. After this the rest followed of course; for I had only to press with my fore-finger and thumb the neck of the animal in the place opposite to the top of the tube, which was forced to descend for some way down the œsophagus,
and

and by a repetition of the same manœuvre I soon brought the tube to the bottom of the stomach, which I knew by the resistance it made when I attempted to push it lower; for now the narrow passage of the pylorus prevented its descent. By a like pressure, but made in the opposite direction, from below upwards, I could bring up the tube from the stomach into the œsophagus, and thence out through the mouth. I employed this contrivance for introducing the tubes into the stomach, and bringing them out at the mouth in water-snakes likewise, and even vipers, managing the last however with some care, which is very requisite, in order to avoid being bit by these serpents during the operation, when they are highly exasperated.

CXVIII. When I was opening some of my land-snakes (*smioldi*) to examine the alimentary canal, I found in the stomach of one a wall-lizard not in the least injured or digested. I thought of employing it for my experiments, as it must be a kind of food well adapted to these reptiles. I therefore enclosed a piece of the tail of this lizard in a tube, which continued for a whole day in the stomach without its contents being at all dissolved. Thirty-six hours produced something more. The tail of the lizard is composed of a number of little muscles, enclased one within the other, and bound round by a thin anular membrane. The piece of tail was placed in the tube in such a manner, that the investing membrane was in contact with the sides, and the muscles were bare at the open ends. The membrane had sustained no injury, but the muscles were eroded on the plane of section, and
a little

a little excavated. Upon touching them I found, that they had been converted into a gluten of some viscidty. The gastric fluid then (for the mechanical action of the stomach could produce no effect within the tubes, were any such action to be exerted at all) had begun to digest the flesh, by dissolving what lay at the ends of the tube before it attacked that which was contiguous to the sides; not only because it was not covered by the membrane, but also because it had freer access at the ends: the solution however went on, though very slowly; for after the tube had been five days in the stomach, a little of the muscular flesh remained, and the membrane was almost entire.

CXIX. The flesh of a lizard's tail is rather tough, and it was probable, that this circumstance had retarded the progress of digestion; it was therefore proper to employ some of a less firm texture; accordingly part of the liver of the same animal was enclosed in the tube, and given to a snake (*Smirondo*). In this instance digestion was more speedy; for in three days and an half the tube was quite empty.

But what if instead of enclosing the flesh in tubes, we should introduce it into the stomach without any covering? It was obvious to suppose, that it would be sooner digested, since the gastric juice would have freer scope for its action. And so in reality it happened. A piece of lizard's tail of the same size as in a preceding experiment (CXVIII), did not require quite two days for its digestion; and a portion of liver, equal to that before-mentioned (CXVIII), underwent the same

same process in thirty-two hours. Of this I assured myself by opening the stomachs of the two snakes, one of which had taken part of the liver, and the other of the tail.

CXX. We come now to the water-snakes or the *natrices*. Nothing can be more striking than the resemblance between the stomach and the œsophagus in this, and the foregoing species. Besides the trachea, lungs, heart, liver, vena portarum, having nearly the same configuration, and lying on the same parts of the œsophagus; this cavity is very capacious and long, consists in like manner of thin membranous coats, and ends in a funnel, which is the true stomach of the animal. The gall-bladder too is about an inch distant from the lungs, and deposits its contents in the duodenum, by means of the cystic duct. The stomach also, as we have observed in the land-snake (*Simirola*), is furnished with a great number of follicular glands.

CXXI. It is easy to learn the nature of the food of water-snakes, and we ought in consequence to provide it for our experiments. Among the antients Oliger Jacobeus, where he treats of frogs, and among the moderns Valisneri will satisfy us, that these reptiles live chiefly upon frogs. Next to man water-snakes may be denominated their greatest scourge. They particularly frequent the water of ditches, puddles, ponds, lakes, such in short as is frequented by frogs; and here they make an easy prey of them, notwithstanding they mutually give each other notice when they perceive the snake at a distance; by a kind of whistle or outcry of distress,

treffs, as I have often observed, at which all fly with the utmost precipitation: Dante was not unacquainted with this circumstance.

Come le rane innanzi l'inimica
Biscia per l'acqua si dileguan tutte,
Finchè alla terra ciascuna s'abbica (a).

A fisherman having brought me three very large and vigorous water-snakes, I gave each at the same time a tube enclosing a different part of a frog; one had muscle, the other liver, and the third spleen. The tubes were left three days and an half in the stomach. Upon forcing them out, I observed the same kind of concoction that I had before seen in frogs (CV, CVI). The flesh was beginning to be changed into an adhesive cineritious gluten; the interior parts were unaltered. The tubes were now introduced a second time into the stomachs, and when they had continued there two days they were found empty; some of the adhesive matter stuck to the outsides of two of them.

CXXII. It is not unknown to naturalists, that this species of snake has no teeth, and is consequently obliged to swallow its prey whole. In summer I have often taken them with whole frogs in the stomach. It was therefore not unreasonable to suppose, that they are capable of digesting the bones; and the less so, as it seems difficult for them to be voided backwards, on account of the narrowness of the intestines. It might indeed be suspected, that these bones are vomited, as I have found to be the case with

(a) *Infern. Cant. 9. Fol. 161.*

the tubes, both in this and the former species; but this is not a constant and regular evacuation, as in crows (LIX) and birds of prey, as we shall see hereafter; but takes place at uncertain intervals, and sometimes does not happen at all for several days. In order however to ascertain the fact, I broke two tibiae nine grains each to pieces, enclosed them in the tubes, and forced them into the stomachs of two water-snakes. After they had continued there two days, they were become soft, and had lost three grains. In five days more they were still softer, and now weighed together only five grains. Soon afterwards the two snakes died, and it was not in my power, though I wished very much to prosecute this curious experiment as far as it would go. From the beginning however of the progress we may suppose, that the bones would have been totally dissolved, and consequently it is highly probable, that water-snakes digest the bones of those animals upon which they feed.

CXXIII. By the activity of the gastric fluid of the water-snake in digesting not only flesh but bone, I was induced to try to procure a quantity that I might examine it more particularly. For this purpose I employed sponges as before (LXXXI, LXXXII), and my success exceeded my expectations. Six little sponges, that had lain two hours in the stomachs of three snakes, enclosed in tubes, afforded me enough to fill a watch-glass of a moderate size. It had the following qualities; the colour approached that of foot, it had the fluidity of water, and evaporated very slowly: it has both a salt and bitter taste, and is not inflammable. Hence it appears to bear
a very

a very strong resemblance to the gastric fluid of the other animals, upon which my experiments have been made: this resemblance extends likewise to the odour, which is exactly like that of the same juices in birds of prey, of which I shall speak particularly in the next Dissertation. I reserve the account of some chemical experiments made upon this fluid, till I shall have an opportunity of speaking of the examination of the other gastric juices which I have already mentioned, or shall have occasion to mention in the present work.

CXXIV. We have before seen the strong analogy between the configuration of the stomach and œsophagus in land and water-snakes. In vipers these cavities have the same general form; nor do they differ with respect to the efficient cause of digestion. I repeated upon them most of the experiments described above: several tubes, furnished with different sorts of flesh, were left in their stomachs for a space more or less long, and the effect was just the same as in water and land-snakes; it would therefore be superfluous to describe them particularly. It will be better to turn the reader's attention to some experiments on these three species of reptiles differently modified, but relative to the same subject.

CXXV. Having frequently opened these animals when newly taken, I have sometimes observed, that their stomachs are not large enough to contain the whole prey, and that part lies in the œsophagus. This part never shewed any mark of concoction, notwithstanding what lay in the cavity of the stomach was sometimes half digested. Thus,

for instance, I have found five or six large beetles in the body of a land-snake or viper; those that lay in the stomach were scarce distinguishable, while, on the contrary, those in the œsophagus were entire, or nearly so. I once saw a frog with the lower limbs, which projected out of the stomach, not at all damaged, while the rest of the body lay in the stomach and was half reduced to a pulp. These experiments made by the serpents themselves gave me reason to suppose, that what takes place in them is exactly contrary to what happens in crows and herons, for the reader will remember, that in these birds the œsophagus is really capable of digestion (LXXVII, LXXVIII, LXXIX, XCIX, C); but in the animals in question it seems to belong exclusively to the stomach. A very simple experiment was sufficient to ascertain the point. Into the stomach of one of these serpents a frog, for instance, might be so introduced that part should lie in the œsophagus. The frog might be fastened to a cylinder of wood, and thus firmly fixed in the same place. The cylinder should touch the bottom of the stomach with its lower extremity, and reach some way above that organ. I applied this apparatus to a water-snake, and at the end of the sixth day opened it longitudinally. Upon examination my inclination to believe that the œsophagus was without efficacy, was changed into firm persuasion. The lower limbs, the part of the animal that had lain in the stomach, had nothing left but the bare bones, whereas the whole body which had extended into the œsophagus had suffered no injury.

CXXVI. The experiments related in the CXVIIth and following paragraphs were made in April, when the animals had lately quitted their subterraneous lurking places, and still retained somewhat of that torpor which benumbs them during winter. At this time digestion, as we have seen, is a very slow process. Are we to presume, that when they become more lively, active, and vigorous, as the heat of the season increases, they likewise perform digestion more speedily? for the effect of heat in promoting the operation of the gastric fluid appears from other facts (LXXXVII). This idea was suggested by reperusing the fine memoirs of the illustrious Trembley on polypes, in which the influence of the temperature of the atmosphere upon the digestion of these wonderful animals is evident; insomuch, that the very food which in a hot season is completely digested in twelve hours, when it is cold requires sometimes two or three days. In order to determine whether the same thing takes place in my reptiles, I chose July for a term of comparison, when the difference, if any existed, must needs be more striking, as the thermometer in the shade stood at 22° and 23° *; whereas in April, when the first experiments were made, it did not rise above the twelfth or fourteenth deg. †. And now upon repeating the experiments already described, I found that heat has some power in accelerating digestion, but not so much as I had

* Eighty-one and an half, and eighty-three and three-fourths, F.

† Fifty-nine, and sixty-three and an half, F.

supposed. Flesh in the tubes did not require above two days to be completely digested; and when an equal quantity was introduced into the stomach by itself, about half that time was sufficient.

CXXVII. Naturalists were already apprized of the tardiness of digestion in serpents. In Bomare we read an account of a serpent at Martinico, which retained a chicken in its stomach for three months, and did not completely digest it, for it still preserved some traces of its shape, and the feathers still adhered to the skin (*a*). It is a circumstance deserving of particular notice, and which I shall have occasion to apply in another place, that flesh does not become fetid from remaining long in the stomachs of these cold animals, as I have observed in the course of my experiments, and especially in a viper, which having been kept above two months in my house, could not but be unhealthy; this individual retained in its stomach for sixteen days a lizard, which had been previously macerated in the gastric fluid; nor could I perceive that it had any odour, except that of this juice. And yet such was the heat of the season, that another lizard, about the same size, which I had placed out of curiosity in a close vessel, containing a little water, emitted an insupportable stench before the expiration of the third day.

CXXVIII. But what can be the cause of this slowness of digestion in serpents? As they are cold animals, that is to say, as their blood very little exceeds the temperature of

(*a*) Dict. d'Hist. Nat.

the air, it may seem probable that this phenomenon might be owing to the want of that heat, which is peculiar to animals of warm blood. And I should not have been unwilling to admit this cause, if other animals, with blood equally cold, had not enjoyed the privilege of digesting their food in a much shorter period, as we shall soon see (CXXXIV). We cannot assign a deficiency of gastric fluid as the reason, for their stomachs abound with it (CXXIII). I cannot attribute it to any thing but the inefficacy of the fluid itself; and this is by no means singular, for we have already discovered a circumstance nearly similar in animals with muscular stomachs, in which the gastric juices do not so soon digest the food, as in animals with intermediate stomachs (CIII).

CXXIX. Among fishes I will first treat of that species which bears so strong a resemblance to serpents, and is even considered in the chain of animated beings, as the intermediate link between fishes and serpents, I mean the eel. The stomach in this animal varies from the structure generally observed by nature; it is not a canal immediately continued with the duodenum, but a kind of blind gut, of considerable length, ending in a point; after the food has been received into this gut, and been digested, it must ascend, and return to the upper part of the stomach, in order to pass into the duodenum, which forms an acute angle with that upper part. The natural figure of both may be seen in Blasius's Anatomy of Animals (*a*).

(*a*) Plate LII. Fig. 1.

Into the stomachs of four eels I introduced several tubes, containing pieces of fish, the food most agreeable to eels. In order to preserve them alive, I turned them in a small stew, whence I could take them at pleasure. They were killed at the end of three days and eighteen hours; and the tubes were found at the bottom of the stomach, entirely covered with a dark-coloured mucus, which, on attentive examination, appeared to be the remains of the fish, that by this time was digested. Upon wiping the tubes, and examining the inside, five out of eight were empty, and the three others contained a bit of flesh of the size of a vetch, but it had lost its cohesion.

CXXX. This experiment abundantly proves, that in this fish digestion is produced by the gastric fluid; I therefore proceeded to experiments upon such as are more justly entitled to the appellation of *fishes*. I chose for this purpose carp, barbels, and pikes, as they were the most easy to be procured. It has been long well known that the alimentary canal in many scaly fishes, is provided with one or more blind appendixes, which, because they lie in the vicinity of the pylorus, have been named *pylorici*; they are always full of a white, mucilaginous, and saltish fluid, which is discharged into the canal, and derives its origin from a number of glands lying in the appendixes. In some species they are few, in others in considerable numbers, and in others again exceedingly numerous; they amount in the sturgeon to an hundred; in those species, in which they are most numerous, the several fasciculi meet in a common duct; hence, notwithstanding

standing their numbers, they discharge their contents into the pylorus (*a*) by a few mouths. In the three species I have just mentioned, this singular apparatus is not to be found; but the inside of the stomach and intestines is furnished with yellow bodies, that probably contribute in some way or other to digestion, though I have not been able to ascertain their precise use. At first sight they look like annular worms, adhering, as in newts, to the internal surface of the stomach (CIX); but if we lay hold of them with the forceps, the annular form vanishes, and we find that they are real appendiculi to the stomach and intestines. When stretched out, they are three lines long; each adheres to the villous coat by a footstalk. If we stretch them till they break, a considerable quantity of yellow liquor issues out, and the body becomes shrivelled; and if it be now removed from the place of its insertion, we find under it a little tumour, through which a globule shines obscurely; and if the tumour be cautiously raised, appears distinctly: it is of a yellowish white colour, from the liquor which it contains. Are these globules clusters of glands, and the vermiform bodies elongated ducts for conveying the liquor into the cavity of the stomach? I would very willingly have adopted this notion, if I had not found that these substances, when compressed from the lower end upwards, never discharge their contents, either from the summit, or any other part, contrary to what happens when we squeeze the folli-

(*a*) Haller, *El. Phys.* T. 6.

cular glands in birds with muscular, intermediate, or membranous stomachs. On this account I suspend my opinion on the subject; although I should incline to suppose that they are of some use in digestion.

CXXXI. Immediately under the teeth, the very beginning of the œsophagus in the carp, is moistened with a considerable quantity of a whitish turbid liquor, of a viscid consistence, and insipid taste, which when wiped off is instantly reproduced: and we here find a number of white papillæ, broad at their basis, and terminating in a point, which, when pressed, emit the same kind of fluid. If we make a gentle pressure any where near these papillæ, a fluid issues out, but, as I should suppose, of a different nature, since it is transparent, thinner than the former, and not at all viscid. With the œsophagus, which is very short, and of considerable thickness, is continued the stomach, of a membranous structure, and very thin. It is easy to distinguish two coats in this organ, the internal and the nervous; in the latter those globules, which left me in doubt whether I ought to consider them as clusters of glands, are buried (CXXX). In this short description, we see sources capable of supplying the stomach with a large quantity of fluid, notwithstanding it wants the *pylorical* appendixes.

CXXXII. The conformation of the stomach in the barbel, does not correspond either with that of the carp, or various other fishes. The œsophagus, stomach, and intestines constitute a single gut, nearly as in earth-worms, and a variety of insects; this gut is only a little dilated at the stomach, and

con-

contracted at the commencement of the intestines. I could not discover within this canal any vestige of glands or analogous bodies. However, both the œsophagus and stomach are continually moistened with a fluid in great abundance; which when we press or dilate either of these cavities, is seen to transude from the internal surface; and since, according to every appearance, it does not arise from glands, we must suppose that it comes from the open extremities of small arteries terminating here.

CXXXIII. The stomach of the pike has the shape of a bag or sack, of much greater length than breadth; it is full of longitudinal rugæ, of a light flesh colour, and composed of coats so thin, as to be semi-transparent. The rugæ extend upwards into the œsophagus, which is easily distinguished from the stomach by its white colour, and greater thickness. There is no appearance of glands either in the one or the other, though both, and especially the stomach, abound with liquor.

CXXXIV. As fishes are subject to vomiting, the tubes which I had introduced into the stomachs of my carps, barbels, and pikes, were frequently returned; and I was often chagrined at finding them, after a few hours continuance in the body, at the bottom of the vessel used for keeping them alive. However, from frequently repeating my experiments, though so many tubes were ejected prematurely, a few remained several hours in the stomach; and these were sufficient to satisfy my wishes. In the present case, the same thing happened which I had so often observed in other animals; the flesh was digested.

gested within the tubes, and that in a much shorter space than in serpents (CXXVI, CXXVII). This observation was verified on the barbels, carp, and pike; the two latter species exhibited a phenomenon, too closely connected with the present subject to be omitted. Happening one day to open a pike, I found within it a little fish, about three inches in length, lying longitudinally along the stomach, so that the whole head occupied the œsophagus. I had here a clear view of the origin and progress of digestion. The jaws of the small fish retained their natural colour, and appeared unaltered. The eye was beginning to quit the orbit, and the gills had lost their purple hue, and were become very soft. In the stomach the marks of digestion were more evident. The flesh of the body was more and more tender as I proceeded downwards; and towards the bottom it had degenerated into a soft and shapeless mass. The extremity of the tail, which had lain at the bottom of the stomach, was entirely consumed, and with it the vertebræ of the spine, and the adjacent bones.

CXXXV. I met with another similar circumstance in a little carp. It had swallowed a small lamprey, which was stretched out at full length, and occupied the whole stomach, and at least two thirds of the œsophagus. The part that lay at the bottom of the stomach was changed into a kind of mucilage, in which there was no appearance of any organized part, except some of the dorsal vertebræ. The parts that lay higher still cohered, but they came away from the animal on being touched. The others, which occupied

cupied the œsophagus, shewed likewise marks of an incipient concoction.

Nothing can be more instructive than these two facts combined. They shew, in the first place, that the bottom of the stomach digests more quickly than the parts situated above, as we have seen in other animals (XC): secondly, that the œsophagus, as well as the stomach, is in some measure capable of concoction, a circumstance that has been already noticed with respect to crows and herons (LXXVII, XCIX, C, CI); and which physiologists have observed before me in other fishes. Lastly, that digestion in the œsophagus is slower at its beginning, and in its progress; two things that have been remarked in the birds just mentioned.

With respect to the triturating power of the stomach in these three species of fishes, not to mention that digestion has been obtained in the tubes without its concurrence, I am of opinion that it has no existence; this I infer, from no effect being produced by it upon the tubes, upon which I have never perceived the smallest bruise, contusion, or injury, in my experiments on fishes, any more than on frogs, newts, and serpents, though they were so thin, that the slightest force would have been more than sufficient to distort or bruise them.

CXXXVI. From cold animals let us proceed to some experiments on the stomachs of warm animals, such as sheep, oxen, and horses. Reaumur, in his second and last Memoir (*a*) concerning digestion, after relating at length

(*a*) Hist. de l'Acad. Roy. An. 1752.

his observations on a kite, slightly touches on some experiments upon dogs and sheep. I will here quote the results of his experiments on the latter species, reserving those on the two others for another place. Desirous of seeing whether digestion in sheep is the effect of the gastric fluid, he forced down the throat of one of these animals four tin tubes, two of which were full of fresh blades of grass, and the two others of chopped hay. Fourteen hours afterwards the sheep was killed and opened, when the four tubes were found in the first stomach, with their contents; and the grass and hay were not in the smallest degree digested, and but little macerated.

Suspecting that they would undergo further alteration, and be even digested by a longer continuance in the stomach, Reaumur caused eight other tubes to be prepared in the same manner, i. e. four to be filled with fresh grass, and the remaining four with hay. The grass before it was put into two of these tubes, and the hay before it was put into two others, were moistened with human saliva. All the eight were forced down the throat of a sheep, which was killed thirty hours afterwards; during this interval, the animal had been kept strictly fasting; this precaution had also been observed with respect to the former sheep, that had not retained the tubes so long. In the course of the thirty hours, the greater part of the tubes were voided at the anus, but a few remained in the first stomach.

But neither had the grass or hay undergone the smallest degree of digestion; they pre-

preserved their original form and dimensions; and when they were pulled at the two opposite extremities, resisted efforts to break them with the same force that similar pieces of grass or hay, that had been a little macerated, would have done. Hence it is inferred by this illustrious naturalist, that digestion cannot be effected in the stomachs of sheep by a solvent, unless that solvent be aided by trituration: he was however ingenuous enough to confess, that these two experiments are of themselves very far from throwing such light upon the present subject as he could have wished.

CXXXVII. The first thing I undertook with respect to sheep, was to repeat exactly Reaumur's experiments. Thinking the tubes I had hitherto employed too small, I had some made eight lines in length, and four in diameter. But I could not at first introduce them into the stomach. After I had put them into the throat with my hand, though I pushed them as far as my fingers would reach, they were always returned. With Reaumur's method I was unacquainted, for he does not give the least information about it. At last an expedient occurred; it consisted in putting the tubes, provided with their contents, into a hollow cane, and introducing this cane into the œsophagus; I could now push them forwards with a rod, till they dropped out at the lower end of the cane into the œsophagus; and as the part of the œsophagus into which the tubes were now introduced, lay at a great distance from the mouth, the animal, in spite of all the efforts he made to return them, was obliged to receive

ceive them into the stomach : to the same contrivance I had recourse likewise in oxen and horses. Six tubes were given to a sheep ; in twenty-seven hours it was killed and opened ; it had eaten nothing during all the time it retained the tubes ; and this precaution was strictly observed upon every sheep upon which experiments were made. Notwithstanding so long a fast, the first stomach contained a large quantity of grass, somewhat triturated ; and though it had fed upon this before the experiment, it was not yet digested. In the midst of this grass, that was thoroughly imbibed with a greenish fluid, with which great part of the stomach was filled, lay five of the tubes ; the sixth had passed to the second stomach, which may be considered as an appendix to the first. The herbs which I enclosed in the tubes, after they had been impregnated with my saliva, were beet, trefoil and lettuce ; in three tubes they were green, and in the rest dry. Upon opening all the six, I could not perceive that either the fresh or the dried plants had suffered any diminution, or undergone any degree of real concoction ; it was only become a little tenderer, and the fresh herbs had lost their green colour ; in short, the result of this experiment was exactly like that of Reaumur's.

CXXXVIII. I should then have supposed, that in these animals digestion depends on the triturating power of the stomach, if it had not struck me, that, as the herbs enclosed in the tubes had not passed further than the first stomach, they might not perhaps have felt the influence of that kind of gastric fluid, which is requisite for the concoction of the

food: for it is very possible that this fluid may reside in some of the other stomachs, and especially in the fourth, in which the aliments of animals with four stomachs, such as sheep, are always found in the state of a very soft paste. Reaumur indeed did not perceive any sign of digestion even in the tubes that had been voided at the anus, and consequently must have passed through the other stomachs. But this observation rested upon a single experiment; and to illustrate still further a matter of such importance, it could not but be proper to repeat it. I therefore treated another sheep in the same manner, and allowed it to live thirty-seven hours afterwards, that the tubes might have time to pass beyond the first stomachs. They did in fact pass beyond them; and I found all six in the fourth, which answered the end I had in view; the three species of plants however, mentioned above (CXXXVII), both the green and the dry, were entire, and seemed only a little softened by maceration.

CXXXIX. I was now about to declare in favour of the necessity of trituration in this animal, when a doubt occurred. Neither Reaumur nor myself had adverted to a circumstance, which ever precedes digestion, both in sheep, and every other quadruped endowed with four stomachs, as goats, oxen, deer, &c. I mean rumination. We are taught, both by dissection and daily experience, that the food, when it has arrived at the second stomach, does not immediately proceed to the third, and thence to the fourth, but, on the contrary, returns, and re-ascends up the œsophagus; and when it has reached the cavity

of the mouth, is masticated and ground over again, and impregnated with a large quantity of saliva; this process is repeated, till it becomes fit to be digested. I therefore entertained many doubts whether the phænomena observed by Reaumur and myself, were not rather owing to the want of rumination than of trituration. Wherefore, in order to decide with certainty concerning digestion in sheep, I perceived it would be necessary to repeat the experiments upon plants previously trituated. And I did not conceive that this trituration was so peculiar in ruminating animals, that it could not be supplied by man, provided he masticated the herbs well, and impregnated them thoroughly with saliva. I therefore performed this easy operation; the usual tubes were employed; in three of them pieces of the green plants were enclosed, and in three others of the dried; but both had been well masticated: the lines and nerves that traverse the leaves, were however easily distinguishable. Left, when thus broken down and divided, they should pass out at the lateral pores, or through the meshes of the lattice-work, I thought it would be proper to enclose each tube in a linen bag; supposing that in the present case that it would not be broken, as that muscular action, which is so considerable in gallinaceous fowls, does not exist in the animals in question. I gave the six tubes to a ram, together with six others, filled with the same plants, but not previously masticated, that I might be able to form a comparison. Fourteen hours after the animal had taken them, he vomited three at once; and in thirty-three hours five more

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were voided at the anus; at the end of two days he was killed. Of the four remaining tubes, two were found in the stomach, and the other two at the end of the duodenum; the bag in which they were enclosed was entire. The tubes that had been vomited had received more or less injury; the contents of two had not been masticated, nor had they undergone the smallest alteration. The contents of the third had been masticated, and were evidently wasted; for they now occupied little more than half the tube, whereas at first they had entirely filled it; they had acquired a subacid taste. Many of the pieces having lost their natural firmness, broke when I attempted to stretch them; the nerves only when they were pulled made some resistance.

In two of the tubes voided at the anus, the pieces of plant had not been masticated; these did not seem at all diminished, or was the cohesion of the parts destroyed; on the contrary, the plants in the other three (which had been masticated) were reduced almost to nothing; the small remains consisted of bare nerves, with a little of the leaf attached; and both the one and the other were so much softened, that the slightest force broke them. The bag in which they were enclosed was dyed green, particularly in its inside; when twisted and pressed between the fingers, it yielded a livid yellow juice, of an acid taste. This was far from being the case in the bags which contained the two tubes, of which the contents had not been chewed; their inside had scarce a shade of green; and this shade was still less perceptible in the juice expressed

from them. With respect to the tubes found in the last stomach, and at the end of the duodenum, the contents of the former had acquired a deep green colour, were a little macerated, but had not lost much of their natural firmness, and did not appear to be diminished in bulk. They had not been masticated, but those of the two others had; and of them there remained only some of the largest nerves, which were themselves very tender, and half decomposed. I have already observed, that the tubes voided at the mouth were more or less bruised; but all the rest were quite free from injury.

CXL. The reader is already aware of the immediate consequences of these experiments. In the first place it appears, that the gastric fluid of sheep has no effect in digesting plants, unless they have been previously masticated; otherwise it can only produce a slight maceration, nearly as common water would do in a degree of heat somewhat exceeding the middle temperature of the atmosphere. Secondly, this fluid is abundantly capable of digesting plants, provided they are previously reduced to pieces by mastication; its first effect is to soften them, and destroy their natural consistency; it then proceeds to dissolve them, not even sparing the toughest parts, such as the nerves of the leaves; of this solution we have a clear proof in the green colour that appears on the linen enclosing the tubes, and in the juice expressed from it (*a*). Thirdly, the triturating power

(*a*) In my agreeable residence at Geneva in the summer of 1779, I had what I had long wished for, the satisfaction

power of the stomach does not at all contribute to digestion in sheep, but this process is entirely effected by the gastric juices. Fourthly, no such power exists in sheep, as we see from the tubes that were voided at the anus and found in the stomach having sustained no injury, notwithstanding pressure alone between

faction of being personally acquainted with my illustrious friend Mr. Bonnet, and of enjoying much of his conversation. I had also an opportunity of taking his opinion on some productions, which I designed to publish, and particularly the present work concerning digestion. Three other reputable philosophers and excellent judges of the subject were present at the reading, Mr. Abraham Trembley, Mr. Johannes his worthy nephew, and Mr. Senebier, Librarian of the Republic of Geneva; and it did not appear to me, that my labours were disapproved by this respectable assembly. Mr. Bonnet gave me a book to peruse on the same subject, which, as it was new to me, gave me fears, lest the author should have anticipated me; it was intitled, "*Essai sur la Digestion, & sur les principales Causes de la Vigueur & de la Durée de la Vie.*" Par Mr. Batigne, M. D. Berlin, 1768, 12mo." But I was soon aware, that Mr. Batigne and myself had pursued very different paths; in his book he does not enter into any experimental enquiry concerning digestion, but confines himself to reflections, which, although they are very pertinent and sensible, are calculated rather to excite than satisfy the reader's curiosity. Hence I should not have mentioned it, but for some objections started against Reaumur's Memoirs on Digestion. These I shall touch upon in a few short notes, at such places of the text as they seem most connected with. And here it is proper to mention one objection relative to the digestion of ruminating animals, which, before I was acquainted with Mr. Batigne's book, I had myself urged against Reaumur, and which experiment proves to be perfectly just. It consists in shewing, that the French naturalist had omitted the mastication of the plants enclosed

tween the fingers is sufficient to flatten them. The contusion of the tubes that were vomited is no proof of the contrary, since it is evident, that this contusion was produced by the teeth of the animal during rumination. Lastly, these vegetables acquire a slight acidity during solution, but of this we shall have another opportunity of speaking hereafter (a).

CXLI. This quadruped feeds not only upon grass, but upon corn also whenever it meets with it; it is likewise very fond of bread. In order therefore to confirm still farther what has been advanced, I thought it would be very proper to make an experiment upon some kind of grain. I selected wheat for this purpose; and as it may be procured under the various forms of seed, flour, and bread, I chose to make trial of all three. Six tubes were filled, three with these substances without any other preparation, and three others with the same after they had been well masticated. The tubes were enclosed in linen bags as before, and given to a lamb six months old. It was killed thirty hours afterwards; none of the tubes were either vomited or

in the tubes before he introduced them into the stomachs of sheep, which was the reason why they were not digested. Nearly the same objection is urged by the learned physician in these terms; "The experiments (of Mr. de Reaumur) upon ruminating animals are still less conclusive; the grass contained in the tubes could be only macerated; since it was neither chewed nor broken down a second time by rumination." (L. c. Troisième Reflexion sur les Experiences de Mr. de Reaumur). It was a piece of justice due to Mr. Batigne not to overlook this passage.

(a) In the last dissertation.

voided

voided at the anus; they were found partly in the third and partly in the fourth stomachs. The result of this experiment coincided with that related above (CXL). The grain, flour, and bread, that had not been masticated, were indeed penetrated thoroughly by the gastric fluid, but not at all dissolved. On the contrary, the corn which I had first bruised with a pestle and then ground between my teeth and reduced to a coarse paste, was in great measure consumed; nothing remained in the tubes but fragments of the bran, with some small remains of farinaceous matter adhering. The like had happened to the flour and bread, what remained consisted of a mucilaginous mass, without any appearance of what it had originally been. This matter had a slight degree of acidity, a quality which was far more evident in the bread, flour, and grain that were not dissolved by the gastric fluid for want of previous maceration.

CXLII. The vast quantity of gastric fluid with which ruminating animals are continually supplied, was already known to physiologists, and particularly to the great Haller. After a fast of two whole days, I have found thirty-seven ounces in the two first stomachs of a sheep. It was green, but I know not whether this colour is natural to the fluid, as the yellow hue to that of crows (LXXXI); or rather whether it is adventitious, and derives its origin from the plants on which these animals feed, of which, notwithstanding so long a fast, there were still some remains in the two stomachs. The great quantity of juice I had collected induced me to try whether, like that of several other ani-

imals, it was capable of digesting food out of the body. I therefore enclosed several pieces of the leaves of lettuce in two short glass tubes (which I had previously filled with the juice), and sealed them with wax at each end. The contents of one tube, as before, were masticated, while those of the other were left untouched. It was proper on the present occasion to employ a term of comparison, by repeating the same experiment upon two other tubes filled with water. That these four tubes might be exposed to a degree of heat nearly equal to the temperature of sheep, I fixed them under my axillæ, two under each axillæ, where they continued forty-five hours. The leaves immersed in the gastric fluid, which had been previously macerated, had undergone no inconsiderable change. Besides the loss of their bright green colour, they were converted into a kind of glue, in which it was just possible to find, with the point of a penknife, a few nerves, which were the only remains of the organization of the plant. This was far from being the case with the leaves that had not been masticated; for all the pieces were distinguishable, and the only difference was, that they did not afford so much resistance as at first. The leaves immersed in water, both those which had been chewed, and those which had not, had not lost either their colour or consistence. From this comparison it appears, that the gastric fluid does not act on the plant as a mere aqueous fluid, but as a real solvent, nearly as it acts in the stomach itself. Nor was the heat to which it was exposed under my axillæ a condition without
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its part in the production of this incipient digestion; for in pieces of the same leaves of lettuce masticated in the same manner, but kept in my apartment, of which the temperature was about sixteen * deg. there appeared only a superficial maceration, notwithstanding they remained immersed in the same gastric fluid for the same space of time.

CXLIII. I closed my enquiries concerning the digestion of ruminating animals by some experiments on oxen. In these the same tubes and plants were employed as before, and the results perfectly coincided with those obtained from sheep; only in the present instance, Nature was more speedy in her operation. Before twenty-four hours had elapsed, the tubes, which had been given to two oxen, were voided along with the excrements; they were not in the least contused or injured. When taken out of the linen bags and examined, they were found to contain little more than the bare ribs and nerves of the leaves of beet, lettuce, and trefoil (which leaves had been previously masticated). The nerves were also in some degree macerated, and the slightest violence was sufficient to break them. On the contrary, pieces of the same plants that had not been subjected to maceration were indeed slightly concocted, and their colour was a little faded, but they were entire. When applied to the tongue, they tasted subacid, like those which had been in the stomachs of sheep (CXXXIX, CXLI).

The horse does not chew the cud, but he resembles the ox in the membranous struc-

* Sixty-six, Fahren.

ture of his stomach, and the food upon which he lives. I was therefore desirous of seeing what changes masticated plants would undergo by continuing a given time in the stomach of this quadruped also, enclosed as usual in tubes. Here too they were digested, as I learned from some lettuce and trefoil enclosed in two tubes, which were voided in fifty-two hours.

CXLIV. When I reflect upon the various animals to which my enquiries concerning digestion have been hitherto extended, I perceive, that the ruminating species very nearly resemble birds endowed with muscular stomachs, with respect to the action of the gastric fluid. In both, that fluid requires an agent capable of breaking down and tritulating the food, before it can dissolve and digest it. From the mouth of granivorous birds, where it undergoes no real alteration, the aliment passes immediately into the craw, where it is softened and macerated; from this receptacle it descends into the stomach: the tritulating power of this organ performs the office of teeth, and breaks, grinds, and, if I may so speak, pulverizes it, and thus renders it fit to be dissolved by the gastric fluid, and converted into chyme. Nature employs a similar contrivance in ruminating animals. The hay and grass descend immediately into the first and second stomachs, in nearly the same state as when they were browsed. Here they are softened by the exuberance of gastric juices, as seeds in the craw of birds with gizzards. But as the stomachs of ruminating quadrupeds have no sensible tritulating power (CXXXIX, CXL, CXLI),

CXLIII), and the aliment requires trituration, nature has wisely provided for this by causing it to ascend, in consequence of a gentle stimulus to vomit, into the cavity of the mouth, where, by means of rumination, it receives the necessary predisposition to be digested by the gastric fluid, as happens to the food in the stomachs of granivorous fowls, after they have been properly triturated by the gastric muscles.

DISSER-

DISSERTATION IV.

THE SUBJECT OF DIGESTION IN ANIMALS
WITH MEMBRANOUS STOMACHS CON-
TINUED. THE LITTLE OWL. THE
SCREECH OWL. THE FALCON. THE
EAGLE.

CXLV. **R**EAUMUR having treated in his first memoir of the mode of digestion in granivorous and herbivorous fowls which are provided with gizzards, in his second proceeds to enquire into the nature of that function in carnivorous birds, of which the stomach is membranous. From the facts related in the first memoir he concludes, that there does not exist in the gizzard any solvent capable of separating the particles of the food. This separation, he thinks, is effected by a force resembling that exerted by mill-stones, viz. the action of stomachs of this construction upon their contents. He is moreover of opinion, that the facts adduced in the second memoir prove the

the existence of a menstruum in membranous stomachs, capable of dissolving and digesting the aliment without borrowing any aid from the action of the solid parts.

As the great object of the first dissertation was to enquire experimentally into the mode of digestion in fowls with muscular stomachs, I had there an opportunity of considering fully Reaumur's experiments on that subject; and we have accordingly seen, that the consequences he has deduced from them are by no means to be admitted in their full extent. This is plain from the XXXIX, XL, XLI, XLII, XLIII, XLVth paragraphs, to which, for the sake of avoiding useless repetitions, I refer the reader. The present dissertation, in which the subject of digestion in membranous stomachs is continued, is the proper place for considering the experiments related in the second memoir. As of all fowls birds of prey approach nearest to man in the structure of the stomach, he chose one of those large kites that are common in France, for the subject of his enquiries. The periodical vomiting, common to all birds of prey, allowed the French naturalist to make a variety of experiments on the same individual. He employed tin tubes filled with different substances, especially flesh, which, after having been some time in the stomach, were thrown up, and gave him an opportunity to examine the effects produced upon the contents. That the flesh was more or less digested according to the length of its continuance in the body of the animal, was the general and invariable result

result observed by Reaumur (a). Hence he justly infers, that in this case digestion is produced by the gastric fluid, without the concurrence of any triturating power. He mentions some other experiments, which I shall have occasion to consider below, and concludes from analogy, that digestion in other birds with membranous stomachs is produced in the same manner. He laments however, that from the death of his kite, and his neglecting to substitute other animals in its stead, he could not adduce facts sufficiently numerous to illustrate the subject fully. He promises to supply the deficiency on some future occasion, but his death, by which a few years afterwards natural philosophy lost its great

(a) Mr. Batigne thinks, that flesh enclosed in tubes was insufficient to convey a precise idea of the alteration it undergoes in the stomach, as it is only macerated in tubes, and not digested. “ On voit de plus que la viande mise dans les tubes ne peut donner une idée précise des changements qu’elle subit dans l’estomac de l’animal, puis qu’elle n’y est que macérée & non point digérée.” (*L. c. premiere Reflexion sur les experiences de M. de Reaumur.*) The author must allow me to observe, that in this attack he misrepresents Reaumur, who, p. 465, &c. of the *Mem. of the Roy. Acad.* expressly says, that the flesh given to the kite was not merely macerated or softened, but completely digested, and at last entirely consumed. He might indeed have objected to the small number of his experiments, as insufficient to ascertain the efficient cause of digestion, if that philosopher, whose ingenuousness was equal to his skill, had not perceived and publicly owned it himself. That tubes, provided the experiments are properly made and varied, are well adapted to shew the change produced upon food in the stomach, will be abundantly proved by the facts adduced in this treatise.

ornament,

ornament, prevented him from fulfilling his promise.

CXLVI. I do not presume that I shall be able to accomplish, what neither this illustrious naturalist, nor any other, as far as I know, has effected. But simply with the view of continuing my observations and reflections on digestion in fowls with membranous stomachs, I shall relate some experiments on various birds of prey, of which some seek it by night, and others by day. Among the former, I have used such as I could most easily procure, the little owl and the screech-owl. The food which I gave the first-mentioned species (a), and which it eagerly devoured, has enabled me to solve, among other problems, one that exercised the sagacity of M. de Reaumur. Finding that the gastric fluid of the kite digested flesh, he wished to know whether it would also digest vegetables; a circumstance he did not think probable, when he considered the repugnance carnivorous birds shew for them; and so in fact it happened. When beans, pease, wheat, inclosed in tubes, had lain some time in the stomach, they were thrown up just in the same state as they had been swallowed: nor did boiling dispose them to be dissolved any better by the gastric fluid. Some sparrows, which I gave my owls, afforded me an opportunity of observing the same phænomenon. As they swallowed them whole, they of course would receive into the stomach feathers and food

(a) This species is called by Buffon *petite chouette*, Hist. Nat. des Oiseaux, T. 2. Ed. in 8vo. and by Linnæus *Strix passerina*, l. c.

not yet digested by the sparrows, and consisting of grain or bread. Now, after the flesh has been digested, the feathers are vomited generally in the form of a hard ball; and along with the feathers the grain, which, though it is much softened by maceration, yet continues whole. And if the matted feathers be disentangled, we may generally perceive evident traces of bread. Hence we have a clear proof that the gastric fluid produces no change on such vegetables.

CXLVII. This fact, simple as it is, shews two things, of some importance to be noticed: first, that the stomach of this bird is really membranous, and without any power of trituration: this appears from the grains (CXLVI) continuing whole, though they had been soaked till they were become so tender, as to burst on being gently squeezed between the fingers. I would not however alledge that the stomach has no action at all; for the globular mats of feathers can only be produced by this viscus contracting as the flesh is digested.

The digestion of the bones also deserves attention. It cannot be said that they are voided along with the excrements; for I must soon have been aware of this, as I kept my owls in cages; nor, for the same reason, could it have escaped my notice if they had been vomited. I have indeed sometimes found two or three little bones, as a dorsal vertebra, or a piece of the cranium, among the matted feathers, but never any thing like the whole skeleton. We must therefore conclude that they are digested.

CXLVIII.

CXLVIII. Reaumur's kite was capable of digesting bone, though of the hardest texture, and enclosed in tubes (a). Though the experiment just related is sufficiently decisive, yet, as the bones were loose in the stomach, to be absolutely certain that the effect was produced by the gastric fluid alone, it was proper to repeat it with a tube; with this view a piece of the thigh of a pigeon was put into one of the same tubes that I used before: thus two experiments, one on the digestion of flesh, and another on that of bone, were made at once. By long practice upon birds of prey, I learned how to keep the tubes in the stomach as long as I pleased. When I had given one of my tubes to an owl, after it had been full fed, I found it was not thrown up till all the food was digested. This observation is applicable to all other birds of prey. The same thing also happened when they were fed sparingly. All the difference was, that as the full stomach requires more time to be emptied, the tubes were retained longer, and *vice versa*. When they were fasting, the tubes were sure to be returned in two or three hours. This observation, together with the knowledge I had acquired from experience, of the time these birds take to digest a given quantity of food, enabled me to guess pretty exactly how long the tubes would continue in the stomach.

I return now to the tube in which part of a pigeon's thigh had been put. After seventeen hours continuance in the stomach, the bone was no where changed except at the

(a) Mem. cit.

broken ends, which were a little softened. The flesh, by which it was covered, as well as the integuments, had begun to be dissolved, for the surface was become exceedingly tender. In fourteen hours more greater effects were produced. The flesh was considerably wasted; the bone was shortened at the ends, and was so soft, as to yield to the pressure of the finger. In twenty-seven hours more, there was no remains of flesh or periosteum, and the bone was a good deal shorter than at first. I could not but be desirous of seeing the end of the experiment, and therefore replaced the bare bone in the tube. When it had remained twenty-one hours in the stomach, it had lost the marrow, and the internal cavity was enlarged, though the girth was lessened. This arose from the corrosion of the internal and external surfaces at the same time. Both surfaces were covered with a yellow fluid, that had at once a bitter and salt taste; and points of gelatinous matter were dispersed over them. The bone, thus half dissolved, was put again into the tube, and left thirty-two hours longer in the stomach. If the reader can conceive a cylinder of thin paper, uneven at the ends, and perforated with several holes, he will have an idea of the state of the bone when it was taken out of the tube. It was covered with the same fluid, which must have been the gastric liquor; and the gelatinous points now also were dispersed over the surface of the leaf; this jelly was the osseous matter itself, reduced to this state by the action of the gastric fluid. Lastly, nine hours longer continuance in the stomach, left only a few small chips. This

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one experiment convinced me, that the gastric fluid of the little owl is capable of digesting bone as well as flesh, without the concurrence of any external agent: it also shews the gradual progress of digestion.

CXLIX. Having so far satisfied my curiosity, it remained to enquire into the nature of this fluid, and its effects out of the animal body. With the small sponges, by means of which I obtained so large a quantity from crows (LXXXI, LXXXII), I procured it in due proportion from the species of owl in question. I say in *due proportion*; for it is evident, that as the stomach of these birds will not admit so many tubes as that of crows, it cannot yield so much gastric fluid. Besides, I had only six little owls, whereas I could get as many crows as I pleased. It was wonderful how soon the sponges were filled with liquor. As they were introduced into the empty stomach, they were soon thrown up, agreeably to an observation made above (CXLVII); and yet they were as full of juice, as if they had been dipped in water; and fresh ones immediately forced down the throat, yielded a nearly equal quantity. I observed the same thing in crows (LXXXIII). Whence it appears, with what care Nature provides a large supply of gastric liquor in these animals, as digestion is entirely dependent upon it. The juice was instantly squeezed out of the sponges into a small glass; it appeared to have the fluidity of water, but was of a reddish-yellow colour, like the yolk of an egg. This colour was not inherent in the gastric liquor; it arose from an immense number of

very small yellow corpuscles, scarce perceptible by the naked eye, but easily seen by help of the microscope. In a few hours they subsided to the bottom, in the form of a yellow sediment, and left the fluid above transparent, like water, where it has been freed from mud that was diffused through it, and rendered it turbid. The first time I saw this phenomenon, I suspected it was owing to some impurities that remained in the stomach, and were mixed with the juice. Before the next experiment, in order to be certain that the stomach was free from heterogeneous substances, I kept the animal fasting for a longer time than usual; but this did not prevent the yellow colour from appearing. Upon opening the stomach of an owl that had been long kept fasting, I could find no foreign substance, but the fluid was as yellow as that squeezed out of the sponges. I was therefore convinced that these particles, though I could not discover their origin, did not come from any remains of the food. The gastric liquor of the little crow, like other gastric liquors, is a little salt and bitter: it evaporated sooner than water. It leaves a sediment of the yellow particles, which gradually becomes dry, and forms a blueish yellow crust; it is not at all inflammable. It has one property common to every gastric fluid I have hitherto examined, or shall have occasion to mention in the sequel; though it is exposed to the open air for weeks and months, in the hottest season, it never becomes putrid.

CL. Such are the properties of the gastric liquor of the small owl, when examined alone.

alone. Let us proceed to the effects it produces on flesh out of the body. In these experiments I used calves intestines, a kind of food which this bird devours very greedily. Forty-six grains were immersed in some recent gastric fluid; and at the same time an equal quantity of the same intestine was put into a phial exactly like the former, and an equal quantity of water was poured upon it. Whenever I have made experiments, with a view to compare the effects of the gastric liquor and water, I have taken care that all circumstances should be alike. To prevent evaporation, the mouths of the phials were stopped with paper; they were set near a kitchen fire, where the usual heat was between thirty and thirty-five degrees. In eleven hours some black spots began to appear upon the intestine in the gastric fluid, which were at first thinly scattered over it, but became gradually more numerous, till in twenty-four hours they almost covered it. During the formation of the spots, I examined the intestine with the microscope, and found that where they appeared, the flesh was softened, and had lost its fibrous texture. When they had spread over the whole piece, I took it out of the liquor, and washed it with pure water; and now it recovered its white colour, for the black covering consisted of a thin stratum of flesh, which the gastric fluid had concocted. It was very easily rubbed off, and fell to the bottom of the water in exceedingly small particles, where it formed a black sediment, and when viewed by the microscope, seemed to be a collection of molecules of flesh, with no appearance of fibres.

When the piece of gut was dried, it weighed only twenty-eight grains, and had therefore lost eighteen; the piece that had stood in water for the same length of time, was quite foetid; whereas the other emitted no disagreeable smell: after washing and drying, it was found to have lost seven grains. Both pieces were again put into the phials, with the same quantity of water and gastric fluid, and left in their former situation for two days. The latter had now lost its shape and organization, and was converted into a black mucilage, of which the particles had no longer any cohesion. The gastric liquor had therefore dissolved the piece of intestine completely; an effect, which neither water nor putrefaction had produced upon the other; for there was a remainder of nineteen grains, that not only retained its fibrous structure, but made considerable resistance when I attempted to tear it.

CLI. I did not neglect to examine the stomach and œsophagus of this species of owl, as I conceived that it would be improper to omit a brief description of these organs in the animals upon which my experiments were made. If the beginning of the duodenum be tied, so as to stop the air from passing, and the upper end of the œsophagus be inflated, we get a view of the œsophagus and stomach dilated to their utmost extent; together they resemble a pear, or rather a gourd, of which the belly is formed by the stomach, and the neck by the œsophagus; when inspected against the light, the latter appears semi-transparent, and the former quite opaque. If they are cut longitudinally,

gradually, and spread upon a table, we find that the transparency of the œsophagus is owing to the thinness of its sides, which thicken as they descend, and render the lower part as opaque as the stomach. It becomes not gradually, but suddenly thicker, from the multitude of follicular glands that form the same kind of transverse fascia, that I have described in other birds; in this species it is about five lines broad. These glands continually secrete into the cavity of the œsophagus a liquor almost insipid, of a turbid white colour, and of some density; in a word, resembling the œsophageal juice of other birds. At the beginning of the stomach the follicles disappear, nor could I find the smallest vestige of any thing like them in the coats, though I searched with care. Are we then to suppose that the source of the fluid, which is always to be found in the stomach, is from the numberless glands lying at the bottom of the œsophagus; this is probably true of part; but that no small part comes also from the arteries of the stomach itself, the humidity, like what I have described in other animals (XCIII, CXXXII), has furnished me with an indubitable proof; for it immediately appears again, after it has been wiped off ever so clean.

CLII. This description will apply to the œsophagus and stomach of screech-owls: I have made experiments on two species of the owl; one variegated with many colours, among which the red and brown, or dull yellow, predominate; upon the head are two curious tufts, in the shape of a crescent; the

other species has not this tuft, but is adorned with a greater variety of elegant colours: the iris is dusky, in the former it is yellow (*a*). My first experiment was made upon one of the long-eared owls, and the result greatly surprized me. It threw up two tubes in about three hours after it had taken them, nor was the flesh at all changed; I could not perceive any alteration, even when it had continued upwards of seven hours in the stomach. If I had not been very cautious in forming opinions, I should have concluded, that the gastric juices of this species are insufficient of themselves to produce digestion; but I reflected, that a single experiment did not warrant such a conclusion, and that some adventitious circumstance might have affected the result. The bird seemed quite stupid, and reduced very much in its flesh; hence it was probably unhealthy, and consequently incapable of digesting its food properly. This suspicion was confirmed by the account of the person from whom I had it, who informed me, that it had refused food ever since it was taken, which was now four days. It was an old bird; and, upon turning to Buffon, I found that, in order to rear individuals of this species, it is necessary to catch them young, for the old ones will not take sustenance in confinement (*b*). In two days and a half longer, that in my possession

(*a*) The former species is called by Linnæus *Strix otus*, and *moyen due* by Buffon; the latter *Strix studula* and *chat huant*.

(*b*) A. l. c.

died without taking any food, and returning what I forced down the throat.

CLIII. This owl fell into my hands in winter; the spring following I procured two young ones from the nest, which devoured food with eagerness whenever hunger pressed upon them: I now repeated my experiment, and the result was exactly the reverse of the preceding; the flesh in the tubes shewed signs of solution in three hours and three quarters, and in seven was entirely dissolved. This convinced me, that the failure of the foregoing experiment was not owing to the inefficacy of the gastric fluid, but to the morbid condition of the animal; which either lessened its quantity, or, what is more probable, impaired its quality. I might, therefore, have omitted mentioning that failure; but it was better to relate it, in order to shew, that when the food inclosed in tubes is not digested, we are not immediately to infer, that the gastric fluid is not capable of producing this effect.

CLIV. But my young owls digested not only flesh, but bone; and that of a hard texture, such as the bones of sheep and oxen, not to mention those of pigeons and fowls. The result was essentially the same as in the preceding species (CXLVII, CXLVIII); instead, therefore, of dwelling upon it, I will relate, at some length, a fact, which, in my opinion, deserves to be noticed. I gave one of my owls a frog, and in an hour killed it. The stomach was exceedingly dilated, and was incapable of containing the whole frog, of which the head lay in the œsophagus, and stretched the sides considerably; the hind
legs

legs lay at the bottom of the stomach, and the flesh was so much wasted, that the bones were nearly bare: the integuments of the thighs and trunk were almost corroded, and the flesh was as tender as if it had been boiled. The head, which lay contiguous to the fascia of follicular glands at the bottom of the œsophagus, had begun to be dissolved. This experiment shews not only that flesh is digested with great quickness by the gastric liquor, but likewise that it is digested equally soon in the œsophagus and stomach; an observation I had not yet made upon any other animal.

CLV. Before I killed both these owls, I was desirous of having some of their gastric juice, that I might see whether it retained like others its power of digestion; and I found, that flesh is completely dissolved by it even when it is assisted by a proper degree of heat.

CLVI. In the other species, the *tawny owl*, the same phænomena occurred with respect to the solution of flesh and bone in the tubes, whether we consider the digestion of flesh and bone in tubes, or the speedy digestion in the œsophagus (*a*), or the remarkable slowness

(*a*) When I was composing the text I was struck by a reflection, for the insertion of which this is the proper place. If we compare the present with the LXXVII, LXXVIII, LXXIX, XCIX, Cth, Cist, CXXXV, CLIVth, it will appear, that the œsophageal before its mixture with the gastric fluid, in many animals, is endowed with some degree of digestive power. This virtue, though it is generally only exerted in conjunction with the gastric fluid, yet in several animals produces its effects in the œsophagus, as we have seen on more than one occasion, when the subject was animals of which the stomach is not capacious enough to hold all the food (which is swallowed with great eagerness), and part must therefore be lodged in the œsophagus.

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of that process out of the body. Upon an individual of this species I made an experiment, which had been unsuccessful on the *little owl*. Observing, that when they were hungry and open their beak very wide, if I dropped a pea, French-bean, or cherry into it they swallowed it with as much avidity as if it had been the pleasantest kind of food, I was desirous of seeing whether the stomach would digest vegetable substances. With this view I enclosed some of the seeds just enumerated in some tubes, and forced them down the throat, but to no purpose; for though the liquor swelled them, and perhaps altered the colour, they underwent no diminution of bulk. They were thrown up undigested in a day or two, a circumstance which sufficiently shews, that such kind of food, notwithstanding they appear to relish it, is ill adapted to their gastric juices. The greediness with which they swallow such substances can arise only from that blind appetite, in consequence of which birds take whatever is offered them.

CLVII. Being satisfied with these experiments on nocturnal birds of prey, I turned my attention to some of the diurnal ones. My first subject was a falcon given me by my illustrious friend the abbe Corti, formerly professor of natural history at Reggio, and now superior of the College of Nobles at Modena, a philosopher well known in the republic of letters by several fine publications. It was of the size of a common hen, and appeared to belong to the species denominated *lanarius* by Linnæus. I soon found, that I could not handle this bird so familiarly
as

as those which I have had occasion to mention hitherto. The strong beak and long sharp talons would not easily permit me to open the mouth by force, and thrust the tubes down the throat. I however contrived a method of introducing them into the stomach without the bird being aware; it consisted in cutting some flesh in pieces, making holes in them, and concealing the tubes in these holes. When the falcon was hungry he ran eagerly to the pieces of flesh, and swallowed them whole. For the fraud to succeed, it was necessary that the tubes should be quite covered with flesh; for if any part of them was bare, the falcon would put them under his talons and tear the flesh away with his beak and swallow it, leaving the tubes.

CLVIII. My first experiment was made with a view to ascertain, whether it was capable of digesting bone independently of the action of the stomach. The result was successful; but I have before said so much on the subject of the digestion of bone, that I should omit relating the present instance particularly but for a new and important phenomenon, which renders the detail necessary. The bone consisted of little splinters of an ox's thigh bone; they were very hard and compact, and of various sizes, from a grain of wheat to a bean; they weighed together sixty-seven grains; I put them into two tubes, in which they were rather closely crammed. To prevent their falling out of the tubes when they began to be dissolved, and consequently to get loose from each other, I put the tubes in a linen bag, a precaution which

which I had before employed, and continued to employ occasionally in future. In twenty-four hours the bones had shifted their respective places and rattled in the tubes, a circumstance that shewed the bulk to be diminished. They were moist with gastric liquor, but had none of those gelatinous points which I had seen in an experiment both on the *little owl* (CXLVIII), and the two other species. These points were, as I then remarked, the osseous matter converted into jelly or chyme by the gastric liquor. But what is extraordinary was, that these splinters retained their original hardness and rigidity; so that at first sight one would not have supposed, that the fluid of the stomach had had any effect upon them. However, the contrary was certain; for when the gastric liquor was wiped off, they weighed only forty-two grains. I now replaced them in the tubes, and examined them again after they had been two days in the stomach. The pieces of the size of grains of wheat were all destroyed but two, which were now no larger than millet. Three of the splinters were at first as big as beans, but now reduced to the size of maize. Those of an intermediate size were diminished in proportion. During the whole time they all continued hard. At the third examination, after fifty-seven hours longer continuance in the stomach, the three large pieces only were left, and they were now no larger than millet; when I struck them with an hammer, I found that they retained their original hardness.

The gastric liquor therefore of the falcon does not, like that of owls and many other animals,

animals, insinuate itself into the substance of the bone, but acts on the surface only. The phænomenon, I think, may be thus explained: conceive a bone to be composed, like wood, or to bring a more familiar instance, like an onion, of a great number of strata. The strata of the onion are of considerable thickness, but we must imagine, that in bone they are exceedingly thin. The gastric fluid of owls or other animals will first dissolve the upper stratum, but while it is doing this it will penetrate and soften the contiguous strata, without dissolving them. Hence the tenderness of bone that has lain in the stomachs of animals. On the contrary, we must suppose, that the gastric liquor of the falcon has no power of penetrating the internal strata, but that its action is limited to the surface. According to this supposition the bone will be digested without having the internal parts softened, and thus stratum after stratum will be taken away, just as it would happen if we had a menstruum capable of dissolving only the superficial layer of an onion without acting upon the others.

CLIX. Before I concluded positively that the gastric juice does not soften bone at all, I determined to observe its effects when it is at liberty to act without any obstacle; for it is possible, that its efficacy might be impaired by passing through the cloth. I therefore took a piece of the same thigh bone from the thickest part, and worked it into a sphere by the lathe, to prevent the angles injuring the fine coats of the stomach; it was then given to the falcon. My purpose
was

was to observe whether as it was dissolved it was also softened.

It continued five days in the stomach without becoming in the least tenderer. The shortening of its diameter shewed that it was lessened in bulk. Meantime the falcon threw up the sphere once or twice a day, according as he was supplied with food; for, as I have observed with respect to other birds of the same class (CXLVIII), he did not vomit indigestible bodies till he had digested the other contents of the stomach. To cause indigestible substances to remain in that cavity after digestion was over, I gave him fresh food; for experience had taught me to judge, when that period was approaching, I was sure to attain my purpose; since when the crop is full of food, the contents of the stomach cannot be evacuated through the mouth. By this contrivance the falcon was made to retain the globe twenty-two successive days. It is scarce worth while to observe, that it was not softened, since the inability of the gastric fluid to produce this effect has been sufficiently proved before; but the remarkable diminution it underwent deserves to be noticed. The sphere was at first four lines and an half in diameter, and when it had been thirty-five days and seven hours in the stomach it measured only a line and about a third; it preserved its form perfectly; the same may be said of its polish; there was not a furrow, nor an indentation, nor an asperity of any sort upon the surface. This smoothness is, I think, a clear proof, that the stomach of this species has no triturating power, otherwise the globe would have sustained some injury

injury from the friction and impulses of so many tin tubes as were introduced into the stomach during its continuance there.

CLX. Let it not however be imagined, that bones of a texture less rigid require so much time to be dissolved; this was very far from being the case. My falcon would eat a whole pigeon at once, for birds of this kind always when they take any large prey fill themselves quite full, and then continue several days without food. My falcon refused the entrails, the tips of the wings, and the beak; the rest he devoured with the utmost greediness. But no bone or flesh was ever vomited, nor did any thing pass out at the vent in the form of bone or flesh; the excrements now, as well as at other times, consisting of a semifluid matter, partly white and partly black. When dry it might be reduced to an impalpable powder by rubbing between the fingers. This animal therefore digested not only the flesh, but the bones of a pigeon, and that in the short space of a day; for at the expiration of this time it would eat a second pigeon.

CLXI. While I was examining the manner in which the falcon digests bone, I was struck with a thought that had never occurred to me during the whole train of the foregoing experiments; it was to enquire whether the gastric liquor besides bone is also capable of digesting some other animal substances, such as the enamel of the teeth, the toughest tendons, and horn. With this view I enclosed two incisors from the lower jaw of a sheep in a tube, which the falcon retained three days and seven hours. Wherever the enamel

enamel did not extend they were corroded and wasted, but the other parts were uninjured, and as brilliant as at first. In four days and an half longer continuance in the stomach the fang was nearly dissolved, but the enamel was perfectly sound. The teeth were kept two days more in the stomach without the tubes, but no further effect was produced; whence it was necessary to infer, that the gastric juice of the falcon is incapable of dissolving the enamel of the teeth; a circumstance which is not very surprising, since it differs from every other osseous substance.

CLXII. I have elsewhere observed, that birds of prey, and consequently falcons, vomit the feathers of the birds which they eat (LIX); it is therefore evident, that the gastric fluid cannot digest them. The smell emitted by burning feathers shews, that they approach the nature of horn; it was therefore reasonable to suspect, that corneous substances would not be dissolved in the stomach, a suspicion which was verified by the event. Some pieces of ox's and sheep's horn were as usual concealed in flesh, and given to the falcon. In a few days they were thrown up entire and uninjured. I have remarked, that the internal coat of the stomach in gallinaceous fowls is not tender and yielding, as in many animals, but firm and cartilaginous (XXXV, XLVII, XLIX, L). Having frequently observed, that when burned it exhales an odour very much like feathers and horn, I supposed that it would in like manner elude the action of the gastric fluid, which really happened; and not only in the thick coats of turkeys and geese, but in the

thin ones of pigeons, blackbirds, and quails. When I gave my falcon the whole stomach of any of these fowls, the other coats were soon digested, but the cartilaginous remained entire.

In tendons the result was different; for my experiment I chose an ox's *tendo achillis*, one of the most tenacious tendons that is to be found in animal bodies. It was hung to dry in summer for several weeks, and thus became so hard, that a keen knife would hardly cut it. However, the gastric liquor of the falcon dissolved it both when it was enclosed in tubes, and loose in the stomach.

CLXIII. Most shoes have the upper leather of calf-skin, and the sole of ox's hide. Both these substances are very readily digested by carnivorous animals when fresh: this at least is the case with the falcon; but the contrary happens when they have been tanned. Another fact has warned me how cautious we ought to be in forming general rules in physics. Who would not have concluded from the last experiment, that every other kind of leather is also indigestible? Yet the reverse happened in sheep-skin dressed, and dyed yellow. Some lists of it were enclosed in tubes, and completely digested in seven hours.

CLXIV. As I had found the gastric fluid of other carnivorous animals incapable of digesting vegetable matters, it was more than probable, that the same thing would take place in the falcon. I however thought, that it would be proper to ascertain this point by experiment, if for no other reason, yet on account of the recent instance of the uncertainty

tainty of analogical arguments (CXLIII). At the same time I was desirous of determining whether digestion is the effect of the gastric liquor solely, as it seemed more than probable. The falcon could very well take six tubes at a time: four were filled with various vegetable substances, such as crumb of bread, chick-pease, slices of pears and apples; in the fifth and sixth were enclosed mutton and beef. Upon these substances the effects of the gastric fluid were exactly the reverse. The flesh was totally dissolved in twenty-seven hours, but the vegetables had undergone no alteration. Two fresh tubes, containing in the middle a bit of flesh, and at the sides masticated bread and boiled pease and chick-pease, decided the question still more clearly. The vegetables were undiminished, but the flesh, which was surrounded by them, was entirely destroyed. Thus the incapability of the gastric juice to dissolve vegetables, and its efficacy on flesh, were fully proved.

CLXV. By means of little sponges I procured this fluid sometimes when the stomach was empty, and at others when it contained some remains of the food, in which case it was always turbid and full of heterogeneous matters, of a cineritious yellow colour, and had not much fluidity. When the stomach was empty it was sufficiently clear, without any extraneous substance, had an intermediate colour between yellow and white, was very fluid, and had a saltish and bitter taste. With this I attempted experiments on digestion out of the body, like those I have before mentioned in several pas-

sages. The result was not different. I obtained the solution of various kinds of flesh by renewing the liquor from time to time, and by applying a heat of thirty deg. the common temperature of these animals. With these precautions I moreover caused nearly the half of a splinter of a bone of beef, weighing forty-four grains, to be dissolved.

CLXVI. Having made these experiments, in my opinion the most interesting the subject admits, my next business was to examine the stomach and œsophagus. However, three hours before I killed my falcon, I fed him, in order to see what effect is produced upon the food in the craw. It was in part in this cavity, and part had descended into the stomach, where it had begun to be decomposed. It was immersed in the gastric fluid, and this incipient digestion had the same appearance as it has out of the body. The flesh in the craw, even that which was upon the point of passing into the stomach, was only a little discoloured; this circumstance shews that digestion is performed only in the latter cavity, and that in the craw the food is only disposed to be dissolved more readily.

CLXVII. When a ligature is made below the pylorus, and air blown in at the top of the œsophagus, this part of the alimentary canal resembles a large intestine about five inches long; a little more than half way down the œsophagus is dilated and forms the craw, though we shall find, that it has this name improperly, if we compare it with the craws of gallinaceous fowls, which lie at the side of the œsophagus, or rather without it;

it; whereas in the falcon the craw is a continuation of that cavity. If we invert and again inflate the œsophagus, and then examine it in a strong light, or with the microscope, we can perceive an immense number of glands from the beginning to the fleshy fascia, not excepting the craw. If we blow in fresh air, and observe it again with the glass, we shall see the glands, which are of an oblong shape, and project a little above the plane of the œsophagus, each emit a drop of liquid; this liquid is so viscid, that one of these drops may be drawn out into a filament an inch or more long; it is insipid to the taste. The most considerable part of the œsophagus, is full of these glands, and is entirely membranous; it only becomes muscular at the commencement of the fascia, which in the falcon, as well as other birds, seems to consist only of numberless follicular glands, and is above an inch in breadth. These follicles are cylindrical, and are all connected by a fine membrane; they have one of their extremities implanted in the external, and the other in the nervous coat of the stomach: through the latter, the excretory ducts open and discharge the same kind of whitish and viscid matter that has several times been described as belonging to birds that have similar follicles. These glands and follicles abundantly supply the stomach with fluid; and though it is sometimes destitute of glandular bodies, yet a liquor continually poured into the cavity by exhalant arteries, forms an addition to that which comes from the œsophagus, as is evident from the moisture which

appears upon the sides when they have been wiped dry several times.

CLXVIII. The eagle on which my experiments have been made, belongs to the species called by Mr. Buffon the *common eagle*, because it is found upon most of the high mountains of Europe; it was known to Aristotle, by whom it is called *Μελαινάετος*, or the black eagle. Hence it has received the denomination of *Falco Melanпетus* from Linnæus, who refers, with whatever propriety, the eagle and falcon to one family. Though some naturalists reckon two species of the common eagle, the brown and the black, I should incline with Buffon and Aristotle to suppose, that there is only one. The difference in colour may depend on the difference of age; for we often see animals of different ages belonging to the same species differ in colour. At the time I was in possession of my eagle I had an opportunity of seeing five others, four dead and prepared, and one living, in the possession of the counts Castiglioni of Milan, two noblemen equally remarkable for politeness of manners and skill in natural philosophy. These animals all varied from each other in colour, some being of a black more or less deep, and others of a darker or lighter brown; yet they agreed in the essential characters of the species. They were all nearly of the same size, somewhat exceeding that of a turkey-cock, their legs and feet were covered with feathers, the nails were black, the feet yellow, the bill blueish, and the base was covered with a bright yellow cere: such are the characters which, according

according to the French naturalist, the brown has in common with the black eagle.

CLXIX. The ordinary food of my eagle consisted of live cats and dogs, when I could procure them. It easily killed dogs much larger than itself. When I forced one of these animals into the apartment where I kept the eagle, it immediately ruffled the feathers on the head and neck, cast a dreadful look at the dog, and taking a short flight, immediately alighted on his back. It held the neck firm with one foot, by which the dog was prevented from turning his head to bite; and with the other grasped one of the flanks, at the same time driving the talons into the body; and in this attitude it continued, till the dog expired, in the midst of fruitless outcries and efforts. The beak had been hitherto unemployed, but it was now used for making a small hole in the skin, which was gradually enlarged; from this the bird began to tear away and devour the flesh, and went on till it was satisfied. I must not omit observing, that it never ate any skin, or intestine, or bone, except very small ones, such as the ribs of cats and small dogs. Notwithstanding this ferocity, and violent impetuosity in attacking animals, it never gave any molestation to man. I, who was the feeder, could safely enter the apartment where the bird was kept, without any means of confining its movements, and beheld these assaults without dread or apprehension: nor was the eagle at all hindered from attacking the living prey I offered it, or rendered shy by my presence. As it was not always in my

power, or at least in my will, to give it living food (for I had not always dogs and cats at hand; and gallinaceous fowls, which are equally acceptable, were too expensive) I substituted flesh which, though it was not so well relished, was not disagreeable. In general, when it had flesh at will, it only made one meal a day. I found, by weighing what it ate, that thirty ounces of flesh served it one day with one another. This species of eagle is provided with a very large craw, which of course is the first receptacle of the food; and when it was at liberty to eat its fill, this viscus was generally distended to a larger size than that of a turkey-cock full of grain. It gradually contracts in proportion as the flesh passes into the stomach, just as it happens in gallinaceous fowls.

CLXX. Some of the first times I observed my eagle eat, I was struck by a phænomenon, which constantly recurred whenever it took food. After it had swallowed a few mouthfuls, a thin stream began to flow from each nostril, and to run down the upper side of the beak; at the end they joined, and formed a large drop, which sometimes fell on the ground, but generally passed into the mouth, and was mixed with the food. This drop was continually renewed by fresh supplies from the nostrils, as long as the animal continued to feed, and after that it ceased to appear. This liquor was of a sky-blue colour, had a salt taste, and was nearly as fluid as water. But why does it flow only while the eagle is feeding? and what is its use? It flows at that particular time only, I suppose,

pose, because the receptacle in which it is contained is then only compressed; and the pressure arises from the motion of the mouth, or the impulse of the food against the palate, near which this receptacle lies. Of the use of this fluid, I candidly own my total ignorance. I suspect, however, that as it is mixed with the food, it serves, like the saliva, to moisten it, and facilitate digestion.

CLXXI. It is commonly thought, and the opinion has the sanction of the best naturalists, that birds of prey, and especially eagles, never drink. What I have observed is, that the species mentioned in the present dissertation, were left even for several months without water; they did not seem to suffer the smallest inconvenience from the want of it; but when they were supplied with water, they not only get into the vessel, and sprinkle their feathers like other birds, but repeatedly dip their beak, then raise their head, in the manner of common fowls, and swallow what they have taken up; hence it is evident that they drink. For the eagle it was necessary to set the water in a large vessel, otherwise, by its attempts to drink, the vessel was sure to be overturned.

CLXXII. To collect into one point of view every thing relative to digestion, let us examine another opinion, more immediately connected with our subject. It is said by several celebrated naturalists and physiologists, that the eagle, when unable to procure flesh, will feed upon bread. To ascertain this point, I made various experiments. I first set before the bird both flesh and wheaten bread; and finding that it ran towards the
flesh,

flesh, without even casting a look upon the bread, I set only the latter before it, and this after a day's fast, when it must have been pressed by hunger; I did not however attain the end I had in view, and therefore kept it fasting for another day, but still to no purpose. When the bread was set near it, it would just look at it, and then turn its eyes towards some other object. When I had prolonged the fast to the fourth day, the bird ran towards me, as I opened the door of the apartment, but with no other view than to ask for food; I offered it a piece of bread, but in vain, for, without even touching it, it returned to the place where it stood before my coming in. I might have carried the trial still further, but was afraid of the animal sinking under it.

CLXXIII. I therefore abandoned this mode of experiment, and thought it would be better to make the eagle swallow some bread; for it would either be always thrown up, and then it would be reasonable to infer, that this was an unfuitable kind of food; or in case it should neither be vomited, nor voided unaltered along with the excrements, and the animal should shew no symptoms of uneasiness, we must conclude that it is digested and assimilated. I concealed the bread in some flesh, as I had done in my experiments upon the falcon (CLVII), and had recourse to the same expedient, whenever I was desirous that my eagle should take tubes or other substances. For though this ferocious bird was exceedingly gentle towards me, who was his feeder, yet it might have been hazardous to irritate it; and that would have been
unavoid-

unavoidable, if I had opened the beak, and thrust bread down the throat by force. The first portion of bread which the eagle swallowed concealed by flesh, amounted to half an ounce. Indigestible bodies, such as feathers, used to be thrown up eighteen, twenty, or, at most, twenty-four hours after they were received into the stomach. But the bread was not vomited in that period, or a day longer; nor did the excrements appear to be altered or mixed with bread. I then gave the animal a whole ounce, instead of half an ounce of bread, none of which was vomited or voided unchanged at the vent. The same thing took place, when the quantity of bread was increased to six ounces. My last experiment upon bread, was to substitute the crust instead of the crumb; but the result was just the same; and, notwithstanding the eagle had shewn so little appetite for this kind of food, its health did not appear to suffer. And I was obliged to conclude, that this species of vegetable is digested, and converted into real nutriment, as well as animal matters. I could not therefore refuse to accede to the opinion of those, who affirm that eagles, when much pressed by hunger, will feed upon bread, though mine would not touch it.

CLXXIV. But in what manner is bread digested in the stomach of the eagle? Is it by the gastric juices alone, or assisted by trituration? Is any such action exerted by it? In short, what is the immediate cause of digestion? These questions are too closely connected with the object of my enquiry, to be passed over unnoticed. To begin then
with

with the first. Tubes employed in my usual manner, would determine the mode of digestion. And in the present case, I observed what I had before observed in so many other animals, that trituration had no part in this function, and that it was the sole effect of the gastric juices. While the eagle retained the tubes, a space that never used to exceed twenty-four hours (CLXXIII), the bread which they contained was completely dissolved. If they happened to remain longer in the stomach, the gastric fluid had corroded the bread, and given it a yellowish colour and a bitterish taste. Where the action of that fluid had been chiefly exerted, the bread was changed into a gelatinous paste, which had nothing of its original taste.

CLXXV. But the tubes shewed, that the gastric liquor of the eagle dissolves not only bread but cheese, at least that sort which in Austrian Lombardy goes under the name of *formaggio piacentino* or *lodigiano*. This power, possessed by a bird properly carnivorous, of digesting a substance so different from flesh, induced me to try whether it is capable of producing the same effect on other matters, and particularly vegetables. But with respect to the latter, I did not find that the efficacy of the gastric fluid extended any further than bread; for several seeds of the *cerealia*, both raw and boiled, did not appear to undergo any alteration in the tubes, or when loose in the stomach. It is somewhat surprising, that this should be the case with wheat, when wheaten bread is so perfectly digested. We see at least, that vegetables must be triturated before

fore they can be digested by the eagle, as well as by gallinaceous fowls (XLV).

The foregoing experiments, and the concurring observations of others (CLXXII), shew, that some animals, supposed to be strictly carnivorous because they live always upon flesh, and are provided with the most formidable weapons for seizing and destroying their prey, may yet, under certain circumstances, change their disposition and manners, and become frugivorous. Thus we read of animals naturally herbivorous, as horses, sheep, oxen, gradually quitting their usual aliment, and learning to live upon flesh (*a*). I too can produce a recent instance in a young wood-pigeon, a species of bird which is universally known to feed upon any thing rather than flesh. By dint of hunger I brought it gradually to relish flesh so well, that it refused every other kind of sustenance, even grain, of which it is naturally so greedy. Such changes, whether effected by design or accident, will not excite the smallest degree of surprize in those who know, that of the various kinds of food used by man and animals, the gelatinous part supplies the nutriment, and that this exists alike in vegetables and animals (*b*). The example of the eagle among carnivorous, and of the horse, ox, pigeon among frugivorous animals, do not however warrant us to conclude, that the former can be universally converted by art or chance into the latter, and reciprocally; for, on the other hand, we

(*a*) Haller. Phys. T. 6. (*b*) Ib. T. I.

have Reaumur's kite (CXVI) and my owls and falcon (CLXVI, CLVI, CLXIV), which were incapable of digesting vegetable substances (a); not that these substances are unfit for affording them nourishment, but because the gastric liquor is incapable of decomposing them, and extracting the nutritious jelly.

CLXXVI. With respect to the second question, whether the stomach of the eagle triturates its contents? I think I have abundant proof, that it possesses no such power. Not to mention the numerous tin tubes that remained so long in it without receiving the slightest injury, I can safely affirm, that I

(a) Mr. Batigne, in his critical reflections on the experiments of Reaumur pretends, that we are not to conclude, because vegetables undergo no change in the stomach of the kite, that the gastric liquor has no action upon them. He supposes, that its inefficacy arose from the vegetables not having been previously masticated. *Premiere Reflexion sur les Experiences de M. de Reaumur.*

But in this Mr. Batigne is mistaken. After I had completed my dissertations on digestion, I procured a kite of the same species as that of Mr. Reaumur, and had it therefore in my power to repeat and vary his experiments. I constantly found, that bread, grain, &c. were thrown up unaltered, both when enclosed in tubes and loose in the stomach, though they had been previously well masticated. This fact agrees with my observation on the falcon, of which the gastric liquor could not digest masticated crumb of bread. I will add, that an owl, fed with chewed bread alone, died upon the fourth day; and upon dissection, the bread was found in its stomach undigested. It is therefore evident, that the incapability of the gastric liquor of some animals to digest vegetables does not arise from the want of previous trituration or mastication, and that this fluid is essentially unfit for dissolving such substances.

could never perceive the smallest contusion upon the grain (which I gave the bird naked in order to try whether it could digest it) (CLXXV), whether raw or boiled; in which case, the smallest compression or impulse would have left evident marks upon the surface. These facts are confirmed by the following observation: I took some strips, about a line in breadth and three inches in length, of exceedingly thin sheet lead, and rolling them up in the form of a spiral, introduced them in some pieces of flesh into the stomach of the eagle, in which they continued eighteen hours. The least force would have sufficed to have destroyed the shape of these strips, and being totally inelastic, they would preserve whatever alteration or distortion they might receive from pressure or percussion. However, when thrown up they retained their spiral form; a clear proof that they had not been subjected to violence of any kind.

Let it not however be supposed, that I mean to exclude motion entirely from the stomach of the eagle. Having frequently found foreign substances within the tubes, and fixed in the perforations, I could not but suppose, that they had been driven into them by some force, and this force could be no other than the agitation of the stomach, which was either extrinsical, and produced by the adjacent viscera, or the peristaltic movement by which the food is expelled through the pylorus. I only assert, that the stomach of the eagle has no action capable of breaking and triturating the aliment, as I think I have abundantly proved. It is likewise clearly ascertained, that the gastric fluid

is the efficient cause of digestion by the experiments made with bread and cheese enclosed in tubes (CLXXV); but this will be more satisfactorily shewn by the experiments relative to the digestion of animal substances, which I am now to relate.

CLXXVII. The first thing I wished to know was what changes flesh undergoes in the craw, and I had therefore to contrive a method of getting it back at pleasure. Had this bird been of the same gentle and peaceful disposition as gallinaceous fowls, this would easily have been effected; for I should have had only to press the portion of flesh that lay highest in the craw upwards with my thumb and fore-finger, and by a continuance of this manœuvre should have brought it out at the mouth. By this simple contrivance I have often examined grain from the craw of fowls, pigeons, and such birds; but the strength and ferociousness of the eagle altered the case totally. After much reflection, I thought of an artifice essentially the same as that adopted for gallinaceous birds. I gave my eagle only three or four pieces of flesh, of which the last was tied in the shape of a cross with a fine pack-thread three or four feet long. The eagle, pressed by hunger, devoured the flesh greedily without regarding the string, of which the greater part hung out of the mouth; nor did the bird make any efforts to swallow or throw it up. When I thought it time to examine the piece of flesh I pulled the string forcibly, and the eagle, without growing enraged, opened its beak and allowed me more room for recovering the string, and by consequence

sequence the flesh that was fastened to it. Sometimes I used considerable force, but did not succeed, probably on account of the flesh being got too low down in the craw; in this case, to free the eagle from the inconvenience, I cut the string close to the beak, and gave it some flesh, which carried down the pack-thread before it into the stomach, whence it was thrown up in a short time; but I have more frequently succeeded in drawing up the flesh, and thus obtained an opportunity of examining it at leisure. I never could find, that the craw or its juices are capable of digestion. Its weight was nearly the same after it was drawn up as before it was swallowed, nor did it seem as if it was upon the point of being digested; the surface was only a little tenderer, and had lost its redness; it was imbibed with a fluid that was neither salt nor bitter, but quite insipid. Flesh therefore is not digested, it is only macerated in the craw of the eagle, as grain and grass in the craw of gallinaceous fowls.

CLXXXVIII. We must therefore conclude, that the whole process of digestion begins and ends in the stomach. If then it was of consequence to know what happens to flesh in the craw, it is of much greater importance to observe how it is altered in the stomach. But as the expedient to get the flesh back, mentioned in the last paragraph, would not be of any service, I contrived another, which answered wonderfully well. I enclosed the flesh I gave my eagle in little nets with small meshes, which were generally vomited empty; but in some there were considerable remains of flesh. The pieces I

used for these experiments were globular, and the remains almost always retained that figure. They were thoroughly impregnated with gastric liquor, and had both a bitter and salt taste. The surface was gelatinous; when this was removed, the fibres were easily distinguishable, but were as tender as if they had been boiled, and the colour was changed to a reddish blue. When this stratum of tender fibres was taken off with a sharp knife, that below was firmer and less discoloured, and at the center the flesh did not appear to have undergone any change either in its consistence or colour. It is needless to observe that these experiments prove, that the gastric fluid dissolves flesh. The permanency of the globular form clearly shews, that trituration does not take place, but the whole effect, to repeat it once more, is produced by the gastric liquor, which acts upon the surface, and dissolves one stratum after another till the whole is consumed, as we have seen the same liquor of other animals act as well upon flesh as other substances (LXV, CI).

CLXXIX. This last experiment rendered it superfluous to try, whether the gastric fluid of the eagle will dissolve flesh enclosed in tubes. Taking this for granted, I proceeded to enquire, whether digestion would be retarded in proportion to the toughness of the flesh with which they were filled. With this view some of the liver, of the muscular flesh of the thigh and heart, a bit of the brain, and a piece of tendon were enclosed in so many distinct tubes. They continued thirteen hours in the stomach; and the gastric fluid acted upon them just as I had imagined.

gined it would. The tube containing the piece of brain was quite empty; of the liver only a very small part remained; the residuum of the muscular flesh of the thigh was more considerable; that of the heart was still greater; but of the tendon there remained most of all. These remains of flesh and tendon had the same appearances as I had observed in the balls of flesh that were introduced into the stomach without tubes. The gelatinous matter on the surface, the tenderness of the fibres lying immediately below, and the consistence of those at the center clearly shewed, that the gastric juices had acted upon the flesh enclosed in tubes just as upon what was left loose in the stomach (CLXXVIII).

CLXXX. My next wish was to know whether its activity would be impaired or destroyed by passing through linen before it got to the flesh. With this view, two pieces of the same tendon and heart, equal in size to those employed in the foregoing experiment, were put into two linen bags, and given to the eagle; in eighteen hours they were thrown up. At first the sides of the bags were distended by their contents, but now that which contained the flesh was a good deal collapsed; for half of it was dissolved: the other had more of its original distension; for not above one-third of the tendon was consumed. Upon comparing together the diminution of the substances in the bags and in the tubes (CLXXIX), I found, that in the former case it was less, notwithstanding the bags continued eighteen hours, and the tubes only thirteen. It is therefore evident, that the

linen is a greater obstacle to the action of the gastric liquor than the tubes.

CLXXXI. From my experiments upon crows (LXVII) it was obvious to conjecture, that as more folds of linen were wrapped round the animal substances, the action of the gastric liquor would be still less considerable. I therefore gave the eagle six bags, containing each an equal portion of beef; the first was single, the second double, and so on. The bird retained them twenty-three hours, when they were all vomited at once, as usually happened to tubes and other indigestible matters, which when small are thrown up all at once, and when large one immediately after another. The two first bags were empty, and the remainder of flesh in the four others were larger as the folds were more numerous, so that in the sixth it was the largest of all. It had however undergone some diminution, and the gastric fluid had therefore begun to dissolve it, notwithstanding the six folds, as appeared from its being impregnated with it, and from the tenderness of the fibres, and the change of colour on the surface. My next wish was to try whether the juices of the stomach were capable of penetrating through a denser substance; I therefore substituted cloth in the stead of linen, and having put sixty-eight grains of beef in the bag, tied some pack-thread very tight round the neck. In fourteen hours it was vomited, and being apparently of the same size as at first, it was returned immediately into the stomach, where it continued twenty-two hours longer. I now found that the cloth, notwithstanding
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its close texture and great thickness which amounted to four-fifths of a line, was thoroughly penetrated by the gastric liquor. The flesh was also moist with it, and appeared, upon being weighed, to have lost twenty-seven grains. Twenty-seven grains had then been dissolved, and as no vestige of them was to be seen in the inside of the bag, it was evident that they must have passed out through the pores of the cloth, and consequently that the gastric fluid is capable of reducing flesh to particles of the utmost tenuity.

CLXXXII. I have before observed, that the eagle devours the smaller bones of dogs and cats along with the flesh (CLIX). When I gave that in my possession a bird, it would also swallow all the bones, except those of the extremities; and as they were not thrown up, there was good reason for believing that they were digested, a circumstance that exactly agrees with my observations on falcons and various other birds (XCVIII, CXLVII, CLIV, CLVIII). But greater certainty was desirable, and this I endeavoured to attain in the following manner: two pieces of the rib of a small dog, each about two inches long, were tied together, and two thigh bones of a cock; this packet was retained twenty-three hours; but the bones were very much altered during that time. The two pieces of rib were reduced to the thinness of a membrane; the least violence was sufficient to break them; they were totally inelastic, and had lost all their marrow. The two thigh bones now resembled tubes of parchment; they were easily compressible, and when left

to themselves recovered their shape, and after being bent they would become strait again. Upon one of the tibiæ thus wasted and altered there was a very singular appearance; about one-fifth was still osseous, but tender, yielding to the touch, and much attenuated. It is therefore apparent, that the juices of the stomach are capable of dissolving bone, and that in a short space. I was unwilling to throw aside these bones thus reduced almost to nothing, and therefore tying them up in a bundle I gave them again to the eagle, in order to see whether they would be entirely dissolved, or, like a caput mortuum, retain their membranous appearance; but being apprehensive that this could not be so well ascertained if they were naked in the stomach, I enclosed them in a tube. It was retained thirteen hours, and upon examination was entirely empty; it was therefore reasonable to infer, that the gastric fluid had now completed the solution.

CLXXXIII. The readiness with which these bones, of a texture by no means tender, were digested, led me to suppose, that the hardest would not resist the action of the gastric liquor. To determine this, I began by giving the eagle a sphere of bone worked at the lathe out of an ox's thigh bone, of the same diameter as that which had been used for the falcon, and taken from the same individual (CLIX). Upon that occasion I observed, that the falcon did not dissolve it during the long space of thirty-five days and seven hours. In the present case it was every day vomited, and immediately returned, and in twenty-five days and nine hours it was completely

completely digested. The eagle is then capable not only of digesting the hardest bones, but of digesting them in a shorter space than some other birds of prey. In the account of my experiments on the falcon I remarked two things, first, that its diameter decreased without any change of shape; secondly, that the texture was not softened during the whole time (CLIX). The first phenomenon occurred on this occasion, the sphere not only maintained its figure, but continued as smooth as when it came from the lathe. But with respect to the second circumstance, there was a wide difference; for notwithstanding the hardness of the bone, the surface was so soft every time it was thrown up, that it was easy to pare off slices with a knife, which were as pliant as cartilage. The gastric fluid then of the eagle, besides dissolving the superficial strata, penetrated into the substance of the bone and softened it; an effect which that of the falcon is incapable of producing. Penetrating, however, as it is, it has no action on the enamel of the teeth, any more than that of the falcon (CLXI).

CLXXXIV. We have seen how much more speedily the gastric fluid of the eagle digests bone than that of the falcon; the same observation may also be extended to flesh. The former bird required thirty ounces a day (CLXIX), the latter was satisfied with twelve, and sometimes with ten. The gastric liquor of the one then dissolves, in an equal space of time, three times as much as that of the other, and consequently the rapidity of digestion in one is triple of that in the other. I should however, upon mature

reflection, be inclined to consider this greater rapidity as apparent, rather than real. The eagle indeed digests three times as much flesh as the falcon in the same time, but then the gastric juice of the former is far more copious than that of the latter; and if we suppose it to be three times as much, a supposition very admissible, as we shall soon see, every third part will dissolve a quantity of flesh equal to that dissolved by the whole gastric fluid of the falcon. The same remark is applicable to other animals. With how small a quantity of flesh is the little owl satisfied in comparison with the eagle, and consequently how inconsiderable is the solution effected by the gastric liquor; but then how trifling does the quantity of that liquor appear when we consider that of the eagle! The same reflection will recur when we compare a lamb with an ox, or a hare with a horse. But with respect to the case in question, I could not devise any more effectual means of determining whether the greater effect produced by the juices of the eagle arose from the greater abundance solely, or in part also from its superior efficacy, than to give each of these birds a small quantity of flesh at the same time, and observe what would be the event. It would either be digested by one as soon as by the other, and then the same efficacy must be ascribed to both; or else the eagle would digest it more speedily than the falcon, in which case the small quantity of flesh would not allow us to suppose, that the fluid of the falcon could not so soon dissolve it on account of its being in smaller quantity, and we must therefore conclude, that it is less
capable

capable of digesting flesh than that of the eagle. This experiment I have often repeated, not only upon the falcon and eagle, but upon the two species of owls also, and crows, and the result has been, that sometimes one and sometimes another of these birds has digested the small portion of flesh soonest; nor did the eagle at all distinguish itself above the rest. As the difference of time was very inconsiderable, it may be overlooked, and we may safely suppose, that the digestive power of the gastric fluid is nearly equal in these several species, and consequently that the eagle has no pre-eminence above the rest. It may however be objected, that with respect to bone, at least, the prerogative of digestion belongs to the eagle in preference to the falcon, which takes above thirty-five days to dissolve the same sphere which the eagle dissolves in less than twenty-six (CLIX, CLXXXIII). I can adopt this opinion without much reluctance, since there is no inconsistency in supposing, that two menstrua may agree in the effects they produce upon one body, but differ with respect to another; nay, this idea is confirmed by the facility with which the gastric fluid of the eagle penetrates into and softens bone, a quality which that of the falcon does not possess in the smallest degree (CLXXXIII, CLIX).

CLXXXV. Let us now proceed to shew the great abundance of gastric fluid in the eagle in comparison with smaller birds, such as the falcon, the owl, &c. To procure this fluid I was not obliged, as in other animals (LXXXI), to use small sponges. The eagle supplied me spontaneously. Very soon after
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it was in my possession I was aware, that along with the tubes a quantity of gastric fluid was thrown up, so that the floor was often quite wet with it. It was easy to devise a method of catching it before it fell on the ground; for the eagle rarely moved from the place where it took food, and therefore generally vomited the tubes on the same spot. Upon this I set a large glass vessel, and thus was enabled to collect a large quantity of liquor, which generally exceeded three-fourths of an ounce a day on those days when the vomiting took place, a quantity which I could not even hope to procure from all the above-mentioned birds of prey taken together. What I obtained in this manner was extremely well suited to my purpose, not being adulterated with heterogeneous matters; for it was always thrown up when the stomach was empty, as I knew by the avidity which the animal shewed for fresh food at this time. Its smell, which I cannot describe, is not disagreeable, but very much resembles that emitted by the gastric liquor of other birds of prey. If we except the colour, which in the others is yellow, but in the eagle cineritious, it exhibited the same qualities, whether we consider the bitter and salt taste, the turbid appearance, which is almost inseparable from the one as well as the other, its fluidity, which nearly approaches that of water, its disposition to evaporate, or the total want of inflammability.

CLXXXVI. The gastric juice of the eagle, as well as that of other animals, is capable of digesting animal and vegetable matters out of the body. It has even produced

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an incipient solution of bone, and an almost complete one of cartilage; but the experiments were made in a considerable heat; for otherwise little or no solution took place, and the gastric juices of the eagle now only prevented these substances from becoming putrid.

Upon this fluid I made two experiments, to which I had not subjected that of other animals. On a very cold day in winter I exposed a small quantity in a glass, on a window, along with two other glasses containing water, in one of which was dissolved a quantity of common salt sufficient to give it a stronger taste than the gastric fluid had. The thermometer set beside the glasses stood at 5 deg. below 0 (*a*). Of the three liquors the first that was frozen was the common water, the next was the salt water, and the last was the gastric fluid. When I carried them into my apartment, where the temperature was three and an half deg. above 0, the first that thawed was the gastric fluid, next the brine, and lastly the water. It must therefore be supposed, that this gastric juice is capable of resisting cold more than common water. As this cannot be attributed to its saline principle alone (otherwise it would have been sooner frozen than the brine), it is necessary to admit some other principle capable of retarding congelation, whether spirituous or oily, or of whatever other nature; and the close analogy subsisting between the gastric liquor of the eagle and other animals

(*a*) Twenty and three-fourths, Fahren.

renders it highly probable, that a like principle exists also in them.

My second experiment was the following: having learned from Mr. Levret (*a*), that the juices of the stomach have the power of dissolving the inflammatory crust of the blood, I procured some of it from a pleuritic patient, and immersed it in a phial of the gastric fluid of the eagle. The event completely answered my expectation; for in two days and a half, in a temperature of 15 deg. the crust was entirely dissolved, and converted into a liquid of a dark hue: this can occasion no surprize; for if the gastric fluid can dissolve animal substances of a far harder texture, such as muscle, cartilage, bone, out of the body, it will much more easily produce the same effect upon the inflammatory crust of the blood.

CLXXXVII. Here the death of the eagle, which happened somewhat more than five months after it had been in my possession, put a stop to my experiments. I however resolved to examine the parts that are situated internally, the only enquiry relative to digestion that could now be made. During the dissection I found, that this individual was a female; for there were many eggs, some smaller and some bigger, attached to the ovaria. It was consequently much larger and stronger than the male of the same species; for it is a constant observation, that the male in birds of prey is about a third smaller and weaker than the female; whereas, in

(*a*) Art d'Accoucher.

other classes, the male exceeds the female in both these respects (*a*). The intestinal canal was full of the usual folds and convolutions; when stretched out at full length, it was about fifty-nine inches long from the beginning of the duodenum to the end of the rectum. There is a double pancreas, and each portion is perfectly distinct and separate; but the same observation has been made upon other animals. Both these glands are of a bluish flesh-colour, of an oblong shape, and smaller towards the end. There is a difference in the size, one being an inch and an half in length, whereas the other is only an inch and three lines. They lie parallel, are situated about five inches from the pylorus, and stretched along beside the duodenum, one on each side, and are attached by cellular substance. At about six inches distance from the pylorus an apparent cord, tinged internally with a dark azure-colour, lies upon the duodenum. If we trace it backwards, we find it gradually enlarged, and at last inserted in the gall-bladder, which, in shape and size, resembles a wood-pigeon's egg. From what has been before observed (LXXXIV, CXV), it is easy to guess the use of this cord; it is the duct through which the bile passes from the bladder into the duodenum. If the gall-bladder be pressed gently the cord becomes immediately tinged with a deeper azure, and the liquor runs into the duodenum: if we open that gut, the upper part is found tinged with a greenish azure

(*a*). Buffon. l. c. T. I.

bile. Upon wiping it away, the entrance of the duct becomes visible, and fresh bile runs into the duodenum when the pressure is renewed. The gall-bladder lies towards the right lobe of the liver, but is not covered by it. The bile is rather dense, and has a strong bitter taste.

CLXXXVIII. When I inspected the stomach I was astonished at its small size, when compared with the crop. The latter cavity is capable of containing thirty-eight ounces of water, whereas the stomach can scarce hold three. We must therefore suppose, that the great quantity of flesh devoured by this voracious bird passes slowly from the crop to the stomach, in proportion as it is digested and expelled into the intestines. Hence it is easy to comprehend how a single meal may serve several days; for a large prey will be equivalent to several smaller ones. I cannot give a better idea of the shape of the stomach, than by comparing it to a man's leg and foot. At the point of the toes lies the pylorus, the foot resembles the bottom of the stomach, and the leg the upper part. The fleshy fascia full of follicular glands, which in other birds, whether granivorous or carnivorous, is situated just above the stomach, in the eagle is contained within its cavity, and makes up the superior and larger half. The internal coat of this fascia is so thin and delicate, that it tears upon being slightly rubbed with a cloth. We come next to the nervous coat full of an infinite number of pores, out of which, when pressure is made, issues a viscid, cineritious, and insipid liquor. Upon removing this coat these pores appear
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to be the excretory ducts of the follicles, of which one extremity adheres to this, and the other to the muscular coat; next the last mentioned lies the external coat, which appears to be membranous. The glands are cylindrical, a line and one-fourth long; they are tied together by a number of membranous filaments. This short description shews the entire resemblance between the fascia of the eagle and other birds. The four coats pass on to the inferior part of the stomach, and extend to the pylorus. The muscular coat seemed to merit a distinct examination. It consists of two strata. That which lies next the nervous coat is formed by fleshy fasciculi, of a lively red colour, running in a longitudinal direction. The other is of a paler red colour, and the fibres intersect those of the other coat at right angles, and of course run transversely. Notwithstanding their nearness they are perfectly separate from each other, like the rings of certain worms, particularly of the earth-worm, which they moreover resemble in their bluish flesh-colour. These two thin strata doubtless cause the various motions of the stomach, of which the effects have appeared in some of the experiments related above. This coat is one-fourth of a line in thickness; upon the fascia it is thinner, and I could only find the transverse stratum; whence it seems probable, that the motion of the stomach chiefly takes place in the lower part, which has no, at least no apparent, glands; but as a thin transparent liquor oozes out on slight pressure, as in the stomach of birds belonging to the same class (XCIII, CLI, CLXVII), we must conclude,

conclude, that it abounds in small arteries, which perform the office of glands.

CLXXXIX. The death of my eagle happened a few hours after it had taken food, nor could I discover the cause. Most of the flesh was in the craw, and a little only had descended into the stomach. It lay at the bottom near the pylorus, but shewed no appearance of being digested, whether on account of the morbid condition of the animal, or because it had but just fallen into the stomach. It was softened by the gastric juices, that tasted very bitter, which, as well as its yellow hue, was owing to the regurgitation of the bile into the stomach, and these qualities were more apparent in the vicinity of the pylorus. The flesh in the crop was not altered in consistence or colour, except that which lay in contact with the sides; this was a little discoloured and somewhat tenderer than at first, circumstances that accord with what was said at the close of the CLXXVIIth paragraph.

Upon emptying, inserting, and then inflating the craw, the surface was covered with a multitude of small drops, which, when united by some flat body passing over them, formed a fluid as transparent and thin as water; it seemed to have a bitterish savour. Upon inspecting the places whence the drops arose, they seemed so many points, which the microscope shewed to be minute pores. Hence it appeared, that every part of the crop abounds with these perforations, which I had no hesitation in supposing to be the excretory ducts of a multitude of glands lying between the coats, as I had also found in the
craws

craws of other birds (XLIX, L, CLXVII). In search of them I dissected away the internal coat, which, in thickness and strength, resembles the nervous coat of the stomach, of which perhaps it is a continuation. But neither in the substance, or between it and the muscular coat, did I find any appearance like glands. All that I could perceive, when I held the internal coat against the light, was the pores already mentioned, that looked like lucid points. Nor did the muscular or the external coats, which last is membranous, contain any glandular body. I was therefore obliged to conclude, that the fluid oozing out in the form of numberless drops upon the internal surface of the craw is secreted, not by glands, but by arteries too small to be conspicuous. The rest of the œsophagus is also full of these pores, and the same fluid issues out from them; no small part of which must run into the cavity of the stomach, and contribute to the formation of the gastric menstruum, which is composed of this and the proper fluid of the stomach, of the bile, and the pancreatic juice.

DISSERTATION V.

THE SUBJECT OF DIGESTION IN ANIMALS
WITH MEMBRANOUS STOMACHS CON-
CLUDED. THE CAT. THE DOG. MAN.
WHETHER DIGESTION TAKES PLACE
AFTER DEATH.

CXC. **T**HE great difficulty with which cats are forced to swallow tubes, and the facility with which they vomit them, hindered me from making experiments upon this irritable animal in the manner I could have wished. Among, however, a vast number of unsuccessful trials I have once or twice succeeded, and thus have been enabled to illustrate one chief object of my enquiries, I mean the efficient cause of digestion. I have used every effort to oblige this animal to swallow bread and flesh, their ordinary food, enclosed in tubes, and in two individuals, one an adult, and the other a young one, have forced them into the stomach. Both were killed, one after having retained tubes filled with

with flesh nine, and the other with bread five hours. The former were found near the pylorus. The outside was wet with gastric juice, the grating at the ends was entire, as also were the tubes, upon which there did not appear any bruise or other injury. Two of the tubes were empty, the third contained a bit of the size of a lentil-feed macerated in the gastric fluid. The center preserved the colour, consistence, and taste of flesh; the surface was changed into a greyish jelly of a bitterish taste.

The tubes containing bread having remained only five hours in the stomach, were not empty. It had been chewed before it was put into the tubes, by which it was moulded into the shape of cylinders six lines and three-fourths long. These cylinders were not completely dissolved, a portion about four lines long remaining towards the middle of the tube, which was externally gelatinous, but internally retained the characters of bread. This experiment then furnishes an irrefragable proof, that the gastric fluid, as well in the cat as in other animals with membranous and intermediate stomachs, is the efficient cause of digestion independently of any tritulating power.

CXCI. If the stomach be inverted and then inflated, it will be covered with humidity, though care should have been taken to wipe it dry. This humidity will appear repeatedly after the stomach has been freed from it, a phenomenon common, as we have seen, to various other animals. It is not possible to discover the pores from which this fluid issues by the aid of a microscope,

nor can any glandular bodies be perceived in the coats, or the intervals between them; but when the stomach is held against the light, and examined with a glass of great magnifying power, a number of bright flat meshes or eyes appear through the coats. I could not however determine the nature of them, notwithstanding I considered the different parts of the stomach with some attention.

CXCII. My success with dogs was much greater than with cats. I could make them take more tubes without being liable to the inconvenience of having them vomited. I could not however force them down the œsophagus, for this operation was attended with the same danger as in the falcon and eagle; whenever I attempted it, the animal used all its efforts to bite me. But as they would swallow them spontaneously, like those birds, I had only to conceal them in pieces of flesh, and throw them upon the floor of the place where the victim of my curiosity was kept. As I always took care he should be hungry, he generally ran towards the flesh, and swallowed it eagerly without mastication; whereas, the cat would keep it in the mouth, and after chewing it for some time, throw out the tubes generally compressed by the action of the teeth, and swallow the flesh only.

I repeated the experiment that succeeded with the two cats (CXC) upon a dog; the animal took six tubes, four full of various kinds of animal substances, as coagulated blood, lights, muscle, and cartilage; the two others contained chewed crumb of bread. In fifteen hours the dog was killed, and the stomach

stomach examined; it contained only four tubes; the other two had not been voided, and I supposed they must have passed on to the intestines, where I found them among the excrementitious matter at the beginning of the rectum. Before I describe the appearances in the tubes, let me say a few words of the juices with which the stomach abounded. As it contained nothing but the tubes, we may consider them as pure. They were of a yellow colour, very bitter, almost without smell, not so fluid as water, and totally destitute of inflammability. These juices evidently consist of two substances, one very thin, the other viscid and gelatinous, appeared from a deposition of the latter after standing a few hours. If the vessel was set near the fire the clear part evaporated, and left a crust which was formed by the gelatinous matter.

The two tubes that had passed out of the stomach were empty, if we except some excrementitious matter that had got in through the meshes of the grating. Of the other four three were empty, nor could I distinguish which had contained bread and which flesh. The tough and compact cartilage alone filled part of the tube in which it had been put, but as far as I could judge by my eye, it was half wasted. It exhibited the same appearances as on a former occasion; it was imbibed with gastric fluid, and had acquired the same taste, at least on the surface. It was so soft, that it seemed to approach more to the nature of membrane than cartilage.

CXCIII. The result of this experiment does not coincide with an observation in the *Prælectiones Academicæ* of the illustrious Boerhaave, commented upon by Haller. The passage is so important, that I must quote it as it stands in the original. “Receptum est in hominum opinione quod ossa animalibus subigantur; cum Helmontianis olim sensit Boerhaavius; ut vero certior esset, curam adhibuit, ut observaret, quid cibis fieret in ventriculis animalium valde cibos coquentium, & experimento cognovit non subigi, Dedit cani devoranda intestina animalium, famelicus erat, affatim deglutiit, subegit minime & per extremum intestinum pendula misere post se traxit. Dedit famelico cani ossa butyro inuncta, reddidit furfura, neque quidquam dissolvit nisi quod in aqua dissolvi potest. Dedit carnes, reddidit fibras carnis exsuccas. Dedit ligamenta, ea post triduum nihil mutata egressit.”

I reserve till hereafter what I have to say on the famous problem concerning the digestion of bone by dogs, and now confine myself to that part of the experiment that relates to the intestine, flesh, and ligament, I must candidly own, that the different results obtained by Boerhaave and myself surprised me. My surprize increased when I considered, that the substances were loose in the stomach of his dog, and of course more liable to be attacked and dissolved by the gastric liquor; whereas, the tubes must more or less impede its access. Upon further reflection it occurred to me, that perhaps the dog was affected by some internal malady, which might alter the properties of the gastric fluid, as in the owl mentioned in the
fourth

fourth dissertation, of which the gastric fluid was rendered unfit for digestion by too long fasting (CLII). This reflection however was not quite satisfactory, and therefore to clear up the matter, I thought it would be better to repeat Boerhaave's experiment, and give a dog some intestine, in order to see what changes it would undergo in the alimentary canal. A middle-sized dog accordingly eat four pieces of the colon and ileum of a sheep, and at the same time took two tubes, containing each a portion of the same intestines. The tubes were voided before the time I had fixed for killing the animal, for both were found among the excrements within about eleven hours after they were swallowed. When the tubes were washed clean, I found that the pieces of intestine were about half digested. The solvent having acted upon both surfaces had reduced their thickness considerably, but what was left still retained its original structure. I then washed the excrements, and discovered several pieces of intestine, wasted indeed, as well as those contained in the tubes, yet easily distinguishable.

CXCIV. This experiment does not exactly coincide with that of Boerhaave; it is not however totally repugnant to it, since the pieces of intestine were not completely dissolved. My long acquaintance with the circumstances attending digestion gave rise to a conjecture, which I resolved to submit to the test of experiment. The digestion of these pieces of intestine, said I to myself, was not complete in the short space of eleven hours (CXCIII); but may it not be so in a longer

longer time? Is not the quantity of solution in some measure proportional to the quantity of time? Does not this appear from undeniable facts related in the foregoing dissertations?

In order to verify my conjecture, I had only to contrive a method to prevent the intestine from passing so soon through the pylorus; and this, I conceived, might be done by enlarging the tubes beyond their usual size. I got the last-mentioned dog to take three such tubes filled with as many pieces of the large intestine of a sheep, as amounted to half an ounce and four penny-weights. The tubes were concealed in pieces of the same intestine. The dog, which as usual, was hungry at the time of the experiment and was still kept fasting, voided some excrement in twenty-one hours, and upon minutely examining it, I thought I had some foundation for believing, that my conjecture was not fallacious; for though some membranous and fibrous fragments appeared among it, which could be nothing but pieces of the intestine in which the tubes were concealed, yet they were much more wasted, and much less easily distinguishable than in the former experiment (CXCHII), on account of their longer continuance in the body. As the process of digestion is less rapid in the tubes than in the open stomach, I let the dog live twenty hours longer, at the end of which the three tubes had remained forty-one hours in the stomach. They all three lay close together at the inferior orifice of the stomach, wrapped up in some bits of rag, which in all likelihood the animal had swallowed before the
experiment,

experiment, and both the tubes and rags were immersed in gastric fluid. I make no mention of this juice, having found it to possess the properties described in the CXCIId paragraph. The reader is more interested in knowing what happened to the intestines contained in the tubes. Nothing could have succeeded better than this experiment: two of the tubes were empty, and what remained in the third did not amount to eleven grains; and thus I had the satisfaction of finding, as I had conjectured, that the incomplete digestion of pieces of intestine sometimes observed in dogs, is no proof of the inability of the gastric fluid to dissolve them; it only shews, that they have not been long enough subjected to its action. Hence the reason of Boerhaave's mistake appears evident. Perceiving some intestine which he had given to a dog hanging out behind, he concluded that the animal could not digest such substances (CXCIId); whereas from the facts just adduced it is obvious, that they had only not continued in the stomach a sufficient length of time.

CXCv. These facts also shew, that flesh in the stomach of the dog loses its fibrous structure, which it retains only when it happens to be voided soon after it has been swallowed. But it might be objected by a rigid partizan of Boerhaave, that I have not strictly proved, that the solution extends to the fibres; for they may have been gradually separated from the common mass, and passing out at the pores, and especially through the meshes of the lattice-work, have left the cavity of the tube empty. I therefore thought
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it would be proper to throw further light upon the subject by a decisive experiment. If pieces of flesh were inclosed in purses of very thick linen, they would either be dissolved and leave no vestige behind them, as by other animals in like circumstances (LXVII, CLXXX, CLXXXI); and in this case we must infer, that dogs are capable of dissolving flesh completely, or else the fibres would remain in the purses, and then we should be obliged to agree with Boerhaave, that the digestion of flesh by dogs consists in expressing the juices and converting them into chyme, while the solid parts remained unaltered. But along with the flesh I subjected harder and more tenacious animal substances, such as tendon and ligament, to the test of experiment. Six bags of very thick linen were given to two dogs; four contained four different sorts of flesh, viz. beef, veal, horse-flesh, and mutton; and the two others tendon and ligament of an ox. Each bag contained a quarter of an ounce; and it is to be observed, that the contents were not cut into small pieces. Being apprehensive lest these bags, though of some bulk, would pass through the pylorus before the time for examining them, I tied to each a bit of dry sponge. They continued four days in the stomachs of the two dogs; but fearing lest so long a fast might be hurtful to the animals, and of course disturb the process of digestion, I fed them several times, though rather sparingly. At the expiration of the time just mentioned, I killed and immediately opened them. The experiment succeeded just as I could have wished; for all
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the bags were in the cavity of the stomach. Suspecting that they might have been torn by the teeth of the dogs, I took particular care to examine, but they were whole, and upon being cut open the four first were as empty as if they had never contained any flesh, but of the tendon and ligament there remained about the size of a hazle-nut; there was no hole in either of the bags. The tendon appeared, upon being weighed, to have lost three-fourths, and the ligament above one half. I examined with particular attention, whether this diminution of bulk and weight arose from the expression of the juices, but the contrary was evident; for they were as pulpy and moist as at first. Hence I had every reason for concluding, that the gastric fluid had really attacked and dissolved the solid parts, so as to enable them to pass through the pores of the linen. The same thing had happened to the flesh. The solution was still further confirmed, by the condition of the external strata of the tendon and ligament, which were become so tender as to be torn by the slightest violence. I was thus fully convinced of the efficacy of the gastric fluid of the dog in dissolving the fibres both of flesh, tendon, and ligament, though the process is less rapid in the latter on account of their greater tenacity and harshness. That Boerhaave's dog should void the ligaments unchanged on the third day (*ea post triduum nihil mutata egressit*, CXCI), if by this expression he means that they retained the nature of ligament, and it seems incapable of any other interpretation, I have not the smallest scruple in believing, having seen the
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same thing in a ligament that had been four days in the stomach of a dog. It had indeed undergone a considerable diminution, a circumstance which the celebrated Dutch physician would also have observed if, instead of judging by his eye, he had taken the precaution of weighing it before it was swallowed, and after it had been voided.

CXCVI. We now come to consider whether dogs are capable of digesting bone; a problem which, if we may rely upon the observations of several celebrated physiologists and physicians, would appear to be decided in the negative. We have already seen that Boerhaave's dog, after having eaten bones dipped in butter, produced no other change upon them than simple water would have done (CXCI). This he also endeavours to confirm by the following remark: "Deinde in stercore canino, quod *Album Græcum* vocant, fragmenta ossium pene non mutata reperiuntur, & sit mera ossium rasura, quæ dentibus canis adrosit, exsuccorum & in unam massam reductorum." It appears from some notes on this passage and his great work (a), that his illustrious disciple, Albert Haller, adopted the same opinion. Dr. Pozzi also asserts in his work quoted above (XIII), that dogs do not digest bone. Of the two experiments which he relates, the following appears the most conclusive. He gave a dog that had been fasting five days three bones which, though dry, the animal swallowed for the sake of the butter with which they were anointed. One of the bones weighed

(a) El. Phys. T. 6.

three ounces, another two, and the third one. In three days they were voided, and had only lost six grains.

Such are the most powerful arguments adduced by physiologists against the vulgar opinion. This opinion however found in Reaumur an able advocate, one who eminently possessed the difficult art of making experiments with success. Of the several productions by which he has signalized himself, none have contributed more to his reputation, than the two beautiful memoirs on digestion, which I have so often quoted with applause. To illustrate the present curious and interesting subject he made the following experiment (*a*). Two compact cylindrical bones, each seven lines long and two in diameter, were given to a small bitch, which was killed in twenty-six hours. The bones that were still in the stomach were diminished in bulk, and it appeared to him that several laminae were taken away. They had moreover acquired the flexibility of horn, though they were at first rigid and inelastic. Hence he infers, that they had been in part dissolved by the gastric fluid.

CXCVII. After having thus noticed the experiments of others, let me relate my own. In the stomach and intestines of the dog, mentioned in the CXCIId paragraph, I found several pieces of bone. They seemed to belong to some quadruped, probably a sheep, and must have been eaten before the dog came into my possession. I did not weigh them, but as far as I could judge by inspection,

(*a*) Mem. 2.

they amounted to above six ounces. Upon washing and then examining them attentively, I perceived certain scars and longitudinal furrows, of which I doubted whether they were produced by the gastric liquor or the teeth of the dog; besides, many of the angles and edges were evidently blunted, at the sight of which the idea of what happens to the hardest bodies in a muscular stomach recurred. I moreover observed, that these places were not so hard as the thicker parts of the bone. But these phænomena only suggested doubts, which I resolved to dissipate by the light of experiment. To obtain this, I filled tubes with pieces of bone, and gave them to a dog. The bones were of various kinds and degrees of hardness: the tubes, which were two in number, were put in a bag of linen, in order to prevent the bits of bone from getting out. To allow the gastric juices a proper time for producing their effects, the dog was suffered to live for seven days, and during this time was fed moderately. One of the tubes, though they were rather large, had passed into the cœcum, and was surrounded by feculent matter; the other remained in the stomach. They were neither of them empty, but their contents, which at first weighed three-fourths of an ounce and eighteen grains, now amounted only to four penny-weights and seven grains. All the angles and edges were destroyed. The softest bones had suffered most. They could now be easily cut in the thin places with a knife. The thinnest parts of the bone were dissolved and had passed through the bag, not a vestige of them remaining. This experiment

ment proves two propositions, viz. that the digestive powers of the dog act upon bone as well as flesh; though the latter, on account of its softness, is more speedily dissolved, and that the gastric juices are the sole efficient cause of digestion.

CXCVIII. This experiment was repeated three times, and the result was essentially the same; but there occurred two circumstances that deserve to be noticed. One of the dogs produced only a small diminution of the bones in eight days, though it was fed plentifully, and seemed to be in perfect health the whole time. This shews, that little or no effect being produced upon the bone, which sometimes happens, as in the cases alleged by Boerhaave and Pozzi (CXCVI), is no proof of the inefficacy of the gastric fluid of this animal; it only shews, that the digestive powers are unequal; nor ought this to excite our surprize, as the same thing is observable in our own species.

The other remarkable circumstance, to which I alluded, is the reverse of the preceding. Among the bones given to one of the dogs, were two dentes incisores from the upper jaw of a sheep. It has been already observed, that the enamel of the teeth receives no injury from the gastric fluids that are capable of dissolving the hardest bones, such as those of the eagle and falcon (CLXI, CXXXIII); yet the gastric fluid of the dog in question, damaged this dense substance. I have now before me the two teeth, which I keep as a curiosity. The enamel of one is corroded in two, and of the other in three places; the five cavities are above a line
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long, and penetrate to the nucleus of the bone. The roots of the teeth were almost entirely destroyed. The powerful menstruum of the stomach, had made greater havock among the other bones that were enclosed along with the teeth; the excavations being wrought in a tenderer substance were more considerable. Upon comparing this phenomenon with the furrows mentioned in the CXCIIIth paragraph, I have no doubt but they were occasioned by the gastric solvent. It deserves to be remarked, that in the case where the enamel of the teeth was destroyed, the linen bag had not sustained the smallest injury, though the solvent necessarily passed through it: nor is this to be wondered at; for we have instances of many gastric fluids that are capable of decomposing the most compact animal substances, though they do not produce any effect on the softest vegetable matters (CXLVI, CLVI). This is also true of chemical menstrua; the nitrous acid dissolves the hardest calcareous stones, but leaves the most friable gypsum and clay untouched.

CXCIX. Though my experiments on dogs decisively prove, that digestion is the effect of the gastric fluid alone; yet it was proper to enquire, whether the sides of the stomach have any motion during digestion, and what that motion is? There were two ways of making this enquiry; mediately, that is, by the effects; and immediately, that is, by opening the abdomen and inspecting the stomach.

With respect to the first mode, though I was certain that the stomach of the dog had no considerable

considerable motion, because neither the tubes nor the bags had sustained any injury; yet in order to see whether it has any motion at all, I gave a dog some thin tubes open at the ends, which were therefore liable to be compressed by the smallest violence. But I could not find the least contusion upon them, after they had been three days in the stomach. The inspection, however, of the tubes, presented a phenomenon which shewed, that the sides of the stomach had not been inactive all the time. Upon opening this viscus I found a mass of hairs, which were of a different colour from those of the dog, and could not therefore have been swallowed while the animal was licking itself. They must have belonged to some other animal that had been devoured by the dog, before it fell into my hands. These hairs were not only floating in the stomach, but many of them had likewise got into the tubes; and this must have been effected by the action of the stomach.

CC. I opened five living dogs, taking care not to wound the stomach. This operation was performed soon after they had taken food; for I presumed that the muscular fibres, irritated by the distension, would contract more conspicuously at this time. The stomach of the first was perfectly quiescent when it was left to itself. But when the point of a knife was drawn over it, the parts that were touched and those that were adjacent immediately contracted, and then returned to their former situation. Upon throwing round a ligature above the cardia and below the pylorus, and taking the sto-

mach out of the body, I thought I perceived a slight peristaltic motion, but it was of short duration. The contraction and dilatation continued to succeed each other in the places that were touched with the knife, or any irritating body, for half an hour. The stomach of the second was not only destitute of spontaneous motion, but was insensible to every stimulus. In the third stomach the peristaltic motion was very conspicuous; the contraction began just below the superior orifice, and proceeded with a gentle undulation to the pylorus, and the dilatation regularly followed. This spectacle lasted for seven minutes. And I could resuscitate the motion by irritating the upper part of the stomach, but it continued only a little while. The peristaltic movement did not appear on the stomach of the fourth dog, but irritation would excite it. And it was in this case confined to the ring or circular band corresponding to the place where the stimulus was applied. This band contracted gently, and the diameter of the stomach was sensibly diminished; in a few minutes it dilated just as slowly. In the fifth stomach the peristaltic motion was as apparent as in the third; it lasted some minutes longer, and when it had ceased in all the other parts of the viscus, a band just above the pylorus continued these alternations. The contraction was so considerable, that the opposite sides of the stomach almost touched each other; but all these motions were exceedingly slow, nor did the sides of the stomach ever dilate or contract suddenly or forcibly.

CCI.

CCI. At the same time I examined the stomach of some cats in the same manner. The result was exactly alike. A gradual movement of contraction and dilatation, beginning at the upper end and extending to the lower, was generally perceptible.

All these experiments, and the reader will find similar ones in Haller, though made with a different view (*a*), clearly shew, that the motion of the stomach of the dog and cat are not capable of triturating the food, but calculated to carry it slowly from the superior to the inferior orifice of the stomach, and thence expel it into the duodenum.

From the great number of dogs that were subjected to these experiments I collected a large quantity of gastric fluid, and found it as capable of producing an incipient digestion out of the body, as that of several other animals mentioned above, both of boiled and raw meat, and likewise of several vegetables. It was however necessary to apply a pretty strong heat, and to change the liquor several times, as in other instances.

CCII. Blasius, in his laborious and accurate anatomy of the dog, says, that the internal coat of the stomach is composed of a congeries of glands (*b*). My opportunities of ascertaining this have been frequent. I have examined it with my naked eye and with the microscope, but could never perceive any glandular appearance. Upon wiping it dry and pressing it, it is covered with an aqueous exudation, but I could not distinguish the

(*a*) Mem. sur les part. irrit. & sensib. T. 1.

(*b*) Anat. Anim.

pores from which this exsudation issues. I have examined several pieces with the solar and the simple microscope, and in some perceived a vast number of lucid points, while in others there appeared nothing of this kind. I then examined the back part, which is contiguous to the nervous coat, and immediately saw, that it is composed of a congeries of oblong particles, of a pale flesh-colour, closely compacted together. These are probably the glands of Blasius; but I cannot affirm that they are really glands, not having been able to distinguish the characteristic marks of glandular bodies in them. But however this may be, it is certain they are destined to transmit a fluid into the stomach; for whenever they are pressed, the above-mentioned exsudation appears upon the internal surface. And this fluid may be expressed several days after the stomach has been taken out of the body.

I have before said, that the pores from which the gastric liquor issues are invisible; but the parts contiguous to the pylorus must be excepted, in which they are very conspicuous. Upon comparing the fluid that thus oozes out with that which is collected in the stomach when it is opened, we shall find a very striking difference. The latter, as we have seen above, is yellow, bitter, and somewhat gelatinous (CXCII). But the former has not one of these properties, being colourless, insipid, and very fluid. Hence it is evident, that the gastric liquor of the dog, that liquor which is the efficient cause of digestion, consists, as in other animals, of several different principles, viz. of saliva, of
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the œsophageal juice, of that which is peculiar to the stomach, of the pancreatic juice, and of bile.

CCIII. To complete my researches on animals with membranous stomachs, it remained to examine that of Man. One may indeed draw very plausible inferences concerning human digestion, from observations on this numerous class; especially from birds of prey, the cat and dog, which resemble us so much in the structure of the stomach. But analogical arguments are probable indeed, but not conclusive. And it is an object of much higher importance to attain certainty in Man than in animals. In the writings of antient and modern physicians no topic is more frequently discussed, yet there is little else besides supposition: direct experiments made upon Man are entirely wanting, and their researches are illuminated only by the twilight of conjecture, and supported by precarious hypotheses. If therefore it was necessary on other occasions to have recourse to experiment, on the present it was absolutely indispensable. Upon reflection it appeared, that the principal experiments were reducible to two heads, viz. to procure human gastric fluid, in order to examine it in the manner that of animals has been examined; and to swallow tubes full of various vegetable and animal substances, in order to see what changes they undergo in the stomach. I will candidly own, that the latter kind gave me some apprehension. The histories of indigestible substances occasioning troublesome symptoms, and being vomited after a considerable

derable time (*a*), occurred to my mind. I also recollected instances where such bodies had stopped in the alimentary canal. Other facts however where the result was contrary, and of more frequent occurrence, gave me confidence. Thus we every day see the stones of cherries, medlars, plums, &c. swallowed and voided with impunity. This consideration at last determined me to make a trial with as great caution as possible.

CCIV. I swallowed in the morning fasting a linen bag, containing fifty-two grains of masticated bread. All the following experiments were made under the like circumstances. I retained the purse twenty-three hours without experiencing the smallest inconvenience, and then voided it quite empty. The string used for sewing and tying it was entire, nor was there any rent in the bag itself. Hence it is plain, that it had not received any damage either in my stomach or intestines. The fortunate result of this experiment gave me great encouragement to undertake others. I immediately repeated it with two of the same bags, with this variation, that one was double, and the other had three folds. My motive obviously was to see, whether these additional folds would impede digestion. The bags were voided in twenty-seven hours, and the double one was empty; but the other still contained a small quantity that had yet the characters of bread.

CCV. From vegetable I proceeded to animal substances. In a single linen bag sixty grains of boiled pigeon were enclosed, and

(*a*) Haller, *Phys. T.* 6.

in another the same quantity of boiled veal; both were previously masticated. The purges were voided in eighteen hours and three-quarters, and the flesh was entirely consumed. Instead of sixty I next employed eighty grains, of which the bulk was not so great as to make me apprehend any danger from its passing down the œsophagus, and still less from its getting out through the pylorus, as at this time it must of necessity be very much diminished in bulk. The flesh had been previously boiled and masticated. I retained it twenty-nine hours, at the expiration of which time there remained eleven grains undissolved. This flesh differed in appearance from that which is taken undigested out of the stomachs of animals. The surface of the latter is gelatinous, but the former was as void of succulency as if it had been set under a press. This appearance, which is analogous to that of the bread in the preceding experiment (CCIV), made me suspect, that perhaps the human stomach might possess a power of compressing its contents, though others of the same structure are destitute of such a power. I therefore determined to bring this suspicion to the test of experiment.

CCVI. Finding that I could digest dressed meat that had been masticated, I wished to know whether I was capable of digesting it without mastication. I swallowed eighty grains of the breast of a capon, enclosed in a bag. The bag was retained thirty-seven hours. So long a space had produced considerable effects, for it had lost fifty-six grains. The surface of the remainder was dry, but the internal fibres appeared to be more succulent.

culent. The piece seemed to have been digested equally, for it retained its original shape.

CCVII. I next wished to know whether this dryness of the surface would take place in raw as well as dressed flesh. I did not doubt but I should digest it in this state more or less speedily; for the human stomach is adapted to the digestion of the one as well as the other, whole nations living upon raw flesh, and raw fish being eaten in some maritime countries; not to mention that oysters, cockles, &c. in the state they are taken, are among the delicacies of the elegant and luxurious, though a food of difficult digestion. I took fasting fifty-six grains of raw veal and as much beef, enclosed in two bags, which were returned about the middle of the next day. Of the veal, as it was the tenderer, there remained fourteen grains, and of the beef twenty-three. In both there was the same dryness on the surface as if the bags had been wrung, or pressed by some external violence.

CCVIII. As then this phænomenon is constant, are we to suppose, that the digestion of flesh and bread, which is produced by the gastric fluid within the bags, is aided by the triturating power of the stomach? Does any such power exist at all? I could devise no better means of dispelling these doubts, than by observing what happens to animal and vegetable substances enclosed in tubes. Should they either not be at all or imperfectly digested, we must infer, that there was wanting some circumstance either necessary, or at least expedient; and we might presume,

presume, that it is trituration. I was then under the necessity of swallowing tubes. Having suffered nothing from the former experiments, I entered upon these without much apprehension. Instead of tin I had my tubes made of wood, fearing lest the residence of the metal in the stomach and bowels should be productive of bad consequences, although I never perceived any in other animals. The gastric fluid had never corroded it, the colour was only turned to black. My wooden tubes were five lines in length and three in diameter. The sides were, as usual, perforated with a great number of holes, in order to allow free ingress to the juices of the stomach, along the whole length of the tubes, as well as at the ends. To prevent the entrance of the fœculent matter during their passage through the long track of the intestines, they were enclosed in linen bags, a precaution not always employed upon other occasions of the like nature. At first I took a single tube, containing thirty-six grains of boiled veal previously masticated. The tube was voided empty in twenty-two hours. The cover of linen was entire, and had prevented any extraneous matter from getting in.

CCIX. This experiment, which is by no means favourable to the doctrine of trituration, induced me to attempt others before I drew any conclusion. As the tube was capable of containing above thirty-six grains, I put in forty-five. I retained it seventeen hours. There was a residuum of twenty-one grains; and now appearances were changed; the veal not only had its natural succulence, but the surface was soft and gelatinous, the

the center alone remaining fibrous. The jelly was sweet, its smell was not at all putrid, any more than that of the residuums in the purges. These appearances were observed in three other experiments with boiled, and one with raw flesh of several different kinds. I hesitated not to conclude, that in Man, as well as numberless other animals, the gastric fluid digests the food without the concurrence of trituration. It is indeed not possible that it should concur; for I have direct proofs, that no muscular action capable of producing such effects is ever exerted by the human stomach. Among the wooden tubes employed in these experiments, I procured some to be made so thin that the slightest pressure would crush them to pieces; and though I frequently used them, not one was ever broken. If I took off the linen cover, which was always entire, and examined them with ever so much attention, I could never perceive the smallest fissure.

CCX. These perfectly coincide with the following facts. Cherries and grapes are said to be voided entire (*a*). I resolved to ascertain by my own experience the truth of these observations. I first swallowed four unripe grapes, because in that state they have greater firmness. In a day they were all voided with the skin whole; the colour was changed from a greyish white to yellow. I next made trial of ripe grapes, which, as every one knows, burst on the slightest pressure. Of twenty-five which I swallowed eighteen were voided entire, of the other seven the skins only ap-

(*a*) Haller, Phys. T. 6.

peared. I made the same experiments with many cherries, as well ripe as unripe, and by far the greater number were voided entire. These experiments, together with those made on the thin tubes, afford the most conclusive evidence, that no triturating force is exerted by the human stomach.

I shall be perhaps asked, what is the cause of the dryness of the fibres, so often observed in flesh enclosed in the linen bags, which would appear to have been forcibly pressed (CCIV, CCV, CCVI, CCVII)? Upon considering the matter I was led to suppose, that the intestines are more concerned in this phenomenon than the stomach. While the bags remain in the stomach, the flesh is converted into a gelatinous matter; for there is no reason for believing this happens in the tubes only, and not in the bags. But when they are protruded into the intestines, they must be surrounded and pressed by the feculent matter. Hence the jelly is squeezed out, and the fibres lose their succulence. And this, not the action of the stomach, I take to be the cause why cherries and grapes are now and then burst.

CCXI. Having thus established this fundamental proposition, viz. that the digestion of flesh and bread is produced in my stomach by the gastric fluid independently of trituration (CCIV, CCV, CCVI, CCVII, CCVIII, CCIX, CCX), I had before me a fine field for experiments that could not fail to suggest some important truth. The necessity of mastication is sufficiently known. There is, perhaps, no person who has not some time or other been subject to indigestion for want of
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having performed this properly. In the course of my experiments I had swallowed some masticated flesh, and some without mastication; but having never taken care that it should be of equal size, I had no term of comparison, and hence was not certain which was most speedily digested. I therefore supplied this omission in the following manner. I took two pieces from a pigeon's heart, each weighing forty-five grains, and having chewed one as much as I used to chew my food, enclosed them in two tubes, and swallowed them at the same time, but without attaining the end I had in view; for the tube containing the chewed flesh was voided in twenty-five hours, and the other in thirty-seven, both empty. Another experiment made under the same circumstances succeeded better, both the tubes were voided in nineteen hours, and I then saw how much digestion is promoted by mastication. Of the masticated flesh there remained only four grains, whereas of the other there were eighteen left. This was confirmed by two other experiments, one made with mutton, the other with veal. The reason is obvious. Not to mention the saliva which moistens and predisposes meat to be dissolved, it cannot be doubted, that when it is reduced to pieces by the action of the teeth, the gastric fluid penetrates more readily, and by attacking it at more points, dissolves it more speedily than when it is whole. This is true of menstrua in general, which always dissolve bodies soonest when they have been previously broken in pieces. This is also the reason why in other experiments, masticated bread and dressed flesh were
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more readily dissolved than unchewed bread and raw flesh. The boiling had made it tenderer, and consequently disposed it to allow ingress to the gastric fluid.

CCXII. It is an opinion common among modern physiologists, that fleshy fibres, tendon, cartilage, and bone lose their juices in the human stomach, but that the solid parts are not dissolved or digested. With respect to fleshy fibres, I must differ from them, having clearly proved the contrary by experiment (CCV, CCVIII, CCXI). As I could bring the other substances to the same test, I would not neglect an enquiry of so much importance. I at first took membrane enclosed in a tube without mastication or division, weighing about sixty-five grains. The tube was voided in thirty-two hours, and presented the following appearances: The membrane was entire, but seemed thinner and shorter. It weighed only twenty-eight grains. This diminution, however, was not a sufficient proof of the solution of the solid parts; it might be the consequence of the privation of the fluids. It was therefore proper to return it into the stomach, and wait the result. The membrane was voided in fifteen hours; it was still in one piece, but exceedingly reduced, weighing now only five grains. This petty remainder I swallowed a third time; the tube was voided in twenty-two hours, and was now completely empty. I afterwards saw the same phenomena in membranes of greater thickness and tenacity; I once digested the aorta of a calf after it had been boiled. The only difference I observed

I observed was, that the compacter membranes required more time to be dissolved.

CCXIII. I made experiments upon cartilage and tendon at the same time. To avoid giving the reader disgust by too particular a recital, I will only mention the bare results. The cartilage was more speedily dissolved than the tendon, the former being totally consumed in eighty-five and the latter in ninety-seven hours. Both were taken from an ox, and had been previously boiled for half an hour.

CCXIV. Bones still remained, and I submitted some both of a hard and soft texture to experiment. The latter were completely dissolved, and required about the same time as cartilage. But the former underwent no perceptible diminution, though it continued upwards of eighty hours in my stomach. I likewise swallowed a naked ball of hard beef bone three lines in diameter, and in thirty-three hours voided it undiminished.

It is therefore certain, that the stomach is capable of digesting not only muscular fibres, but membrane, tendon, cartilage, and even bone itself, provided it is not too hard; though most physiologists and physicians have been led to adopt a contrary opinion by observing, that these substances are evacuated unaltered. But this is no proof that they are indigestible (for if they had made the experiment on themselves, and weighed the substances, they would have observed a waste), it only shews, that they are not so soon digested as other kinds of food, which are dissolved in a few hours; whereas, membrane,
tendon,

tendon, cartilage, &c. require several days, on account of their tenacity and hardness.

Let no one suppose, that my stomach, being stronger than common, is capable of digesting what that of others cannot digest. I own, with concern, that it is weak, as is usual in those whose pursuits condemn them to a sedentary and unwholesome way of life. My stomach digests food so slowly, that I cannot study for five or six hours after a sparing dinner, and am liable to indigestion whenever I feed more plentifully than common.

Before I quit this subject let me observe, that though I have mentioned the gastric juices as the efficient cause of digestion in the experiments on myself, yet I mean not to exclude those of the intestines from their share. We know, that the small intestines complete the process of *chylification*, which is but begun in the stomach. I must therefore allow, that the digestion of animal and vegetable substances in the bags and tubes is perfected in the intestines. But this is not in the least repugnant to the result of those experiments that shew the human stomach to be destitute of any tritulating force, and digestion to be the effect of the gastric fluid alone, though the fluid which is secreted by the sides of the small intestines may complete the process.

CCXV. In the CCIIId paragraph I remarked, that the chief experiments on man were reducible to two heads, those which relate to the natural process; as it may be observed by means of tubes and such contrivances, and those which relate to artificial digestion

digestion, provided the gastric juices can be procured. Having treated the former of these divisions as well as circumstances would permit, it remains for me to make some enquiries relative to the second. It was first necessary to devise a method of procuring the gastric fluid. The first idea that struck me was to search for it in dead bodies, but after examining several stomachs I was obliged to abandon this search; for they were either without any fluid, or else what they contained was so turbid and so much adulterated with heterogeneous matters, that it would by no means suit my purpose. Nor were the little sponges, which had served so well in animals, better adapted to the present occasion. Two sponges would not supply me with a sufficient quantity, and I could venture only to swallow two tubes at once, for fear of forming an obstruction in my stomach. Besides, the juice thus procured would have been very impure, on account of the heterogeneous matters that the tubes must necessarily have imbibed during their passage through the intestines.

There remained only to obtain it by exciting vomiting while the stomach was empty. To effect this, I chose rather to tickle the fauces than drink warm water, as in this case the gastric fluid must have been diluted. In this manner therefore, before I took meat or drink, I procured in two mornings a quantity sufficient for a few experiments, of which the result shall be related below. I could have wished to have made a greater number, but the disagreeable feelings occasioned by the act of vomiting, the convulsions of my whole
frame,

frame, and more especially of my stomach, that continued for several hours after it, left upon my mind such a repugnance for the operation, that I was absolutely incapable of repeating it, notwithstanding my earnest desire of procuring more gastric liquor.

CCXVI. I was therefore obliged to content myself with what these two vomits afforded me. The first time it amounted to an ounce and thirty-two grains. It was frothy at its being thrown up, and somewhat glutinous. After it had been at rest a few hours and deposited a small sediment, it was as limpid as water. It was a little salt to the taste, but not at all bitter. It did not either, when thrown on the fire or brought near a candle, shew any token of inflammability (a). It

(a) From this and the LXXXIst, CXXIIIrd, CXLIXth, and CLXXXVth paragraphs we may collect, that the gastric juices both of man and animals are destitute of inflammability. I made these experiments, because Reaumur thought that that of his kite was inflammable, which quality Dr. Batigne imputes to the bile, a fluid consisting principally of oil (*premiere Reflexion sur les Exper. de Reaumur*). But were this true, the gastric juices of most of my birds ought to have taken fire. As all mine are contrary to Reaumur's single experiment, I should suspect, that what he observed was owing to accident. His experiment was the following: To take away the smell of putrid flesh, which one of his tubes had acquired, he set it upon some burning coals, when immediately there issued a flame from the inside that lasted above a minute (*Seconde Mem.*). But it is easy to perceive, that this might have been owing to some fat of the flesh enclosed adhering to the tube. I am more confirmed in this suspicion from having observed, that the gastric fluid of a kite, such as Reaumur's, mentioned in a note to paragraph CLXXXV. was not more disposed to take fire than the other gastric juices which I examined.

evaporated in the open air, and when I put fifty-two grains into a vessel and set it on hot coals, it emitted a thick smoke. Another small portion, weighing eighty-three grains, was put in a phial, which was closed with a stopple to prevent it from evaporating. It did not change colour or taste, nor did it acquire any bad smell, notwithstanding it was kept above a month in the hottest season of the year. I thus employed about one half, the remainder was used for an attempt to obtain artificial digestion. It was put into a glass tube two inches long, sealed hermetically at one end, and very narrow at the other; I then introduced a small quantity of masticated boiled beef, and stopping the tube with cotton, set it in a stove close to a kitchen fire, where there was a considerable heat, though not perhaps exactly equal to the temperature of my stomach. By the side of this tube I placed another, containing the same quantity of flesh immersed in water. The appearances in both were the following: In twelve hours the flesh in the former began to lose its fibrous structure, and in thirty-five it had so far lost its consistence, that when I attempted to lay hold of it, it slipped from between my fingers. But though to the naked eye it appeared to be reduced to a pul-taceous mass and to have lost its fibrous texture, yet the microscope rendered the fibres visible; they were however reduced to a great degree of minuteness. After this semifluid shapeless mass had continued two days longer in the gastric fluid, the solution did not seem to have made any further progress, and the reduced fibres were still just as apparent. The
flesh

flesh did not emit the least bad smell, while that immersed in water was putrid in sixteen hours, and became worse and worse the two following days. It lost in some measure its fibrous structure, as always happens during putrefaction; but this appearance did not proceed so far as in the other portion, for the fibres were entire on the third day.

CCXVII. I vomited the second time more gastric fluid, and was now enabled to examine it again as I had done before; and it appeared to possess exactly the same properties. In order to determine the influence of heat two tubes were filled with it, and some flesh was immersed as before (CCXVI). One of the tubes was placed in the stove, and the other left in the open air. In the former the flesh was just as much dissolved as in the preceding experiment; but in the latter the solution proceeded no farther than when water was employed (CCXVI). There was however no putrid smell, though the flesh was left immersed in the gastric fluid seven days.

Before I conclude this account, I must mention a circumstance that happened the second time I procured gastric liquor by vomiting. Four hours before I submitted to this disagreeable operation, I had swallowed two tubes filled with beef, one of which was thrown up; the flesh was thoroughly soaked in the fluid of the stomach, and the surface was soft and gelatinous; it had moreover wasted from fifty-three to thirty-eight grains. This experiment proves, that there is a considerable degree of digestion in the

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stomach,

stomach, before the food passes into the intestines.

CCXVIII. We may now safely lay down some general consequences concerning digestion in Man and animals. In the experiments on birds with muscular stomachs, we have seen how trituration disposes the food to be digested. Hence Nature has furnished this class with gastric muscles of sufficient power to effect this necessary preparation. But we have likewise seen how digestion, which consists in the transmutation of the aliment into chyme, is the effect of the juices alone with which the stomach abounds (Diff. I).

We next proceeded to birds with intermediate stomachs, such as crows and herons, and found, that in them digestion was owing to the gastric fluid alone (Diff. II).

We next considered animals with membranous stomachs, a class so numerous and various, that it comprehends almost every family of living creatures; it includes the inhabitants of salt and fresh water; amphibious animals, as the frog, the newt, and water-snake; reptiles, as the viper, the land-snake, and many others; quadrupeds, as the cat, the dog, the horse, the ox; birds, as birds of prey: to this catalogue Man himself is also to be added.

In several of these animals we have seen the necessity of previous trituration, as in the ruminating order and in Man; in them it is produced by the teeth, as in gallinaceous fowls by the muscles of the stomach. But in others, as in the frog, the newt, serpents, and birds of prey, it does not at all
contribute

contribute to digestion. But in the latter, as well as the former cases, we have seen how the food is dissolved and digested by the gastric fluid (Diff. III, IV).

In every order of animals, Nature, ever uniform in her operations, employs one principle for the performance of this vital function. Hence she has so copiously furnished the œsophagus and stomach with glands, follicles, and other contrivances that answer the same end, whence continually flow the juices so necessary to the life of Man and animals. These juices agree in many properties, but the difference of effect shews, that they differ in others. In the frog, the newt, scaly fishes, and other cold animals, the gastric fluid produces digestion in a temperature nearly equal to that of the atmosphere. But the gastric fluid of hot animals is incapable of dissolving the aliment in a degree of heat lower than that of the animals themselves. There is also a difference in celerity of action, and in efficacy. In celerity, because the food in hot animals is digested in a few hours; whereas, in the opposite kind it requires several days and even weeks, particularly in serpents. In efficacy, because the gastric juices of some animals, as the gallinaceous class, can only dissolve bodies of a soft and yielding texture, and such as have been previously triturated; while those of others, as serpents, the heron, birds of prey, the dog, decompose substances of great tenacity, as ligament and tendon, and of considerable hardness, as the most rigid bone. Man belongs to this division; but his gastric fluid seems to have no action on the

hardest kind of bones. Further, some species, as birds of prey, are incapable of digesting vegetables. But Man, the dog, the cat, crows, &c. dissolve the individuals of both kingdoms alike. In general these juices produce their effects out of the body, as the numerous instances of incipient digestion under this circumstance, both with the gastric fluid of animals and Man abundantly shew.

CCXIX. Having thus brought into one point of view the principal circumstances relative to the efficient cause of digestion, let us compare them with what has been most plausibly written upon this topic so interesting to the physician. The opinion that prevails chiefly in the schools of Europe is that advanced by Boerhaave, who has in truth done nothing but reconcile the opinions that had been proposed at different times before him. He observes, in the first place, that the various solid and fluid substances which serve for food, being received into a close, moist, and warm vessel, must, according to the nature of each, sooner or later begin to ferment or putrefy. There are also various fluids continually running into the cavity of the stomach, viz. the saliva, the œsophageal liquor, that thin transparent fluid which is secreted by the gastric arteries, and a viscid humour secreted by glands in the stomach. If we consider the properties of these ingredients, and moreover take into the account the remains of the food which serve as a ferment, the air which produces an intestine movement of the integrant parts, the heat which excites this heterogeneous mass, we shall find, that the aliment will be macerated, diluted,

diluted, attenuated, dissolved, determined to an incipient fermentation, and in short, impressed with the primary principle of vitality. Thus it is that Boerhaave explains the digestion of soft food. With respect to that of a firmer texture, imagining, that the causes above recited are insufficient to explain the digestion of them, he has recourse to the triturating power of the stomach, produced by the action of the muscular coat, and the pulsation of the aorta and the other adjacent arteries; the nervous fluid, which perhaps flows into the stomach more copiously than elsewhere; and lastly, the continual and strong compression of the diaphragm and abdominal muscles. In consequence of these additional causes, in the first place, the food will be broken down into a pulp, and acquire a cineritious hue; secondly, the fibres, tendons, cartilages, &c. will be deprived of their juices while they retain their cohesion; thirdly, from vegetable and animal substances thus dissolved, will be produced a fluid resembling our humours.

CCXX. Thus has this celebrated physician explained his ideas concerning digestion in his Institutions. He supposes, that there are two principal agents in this vital function, viz. the different fluids that are collected in the stomach, and the mechanical action of that organ; the secondary agents are heat, air, the nervous fluid, the remains of the food, and an incipient fermentation.

With respect to the gastric fluid, his ideas were indeterminate and unsettled. On comparing this passage with his Prælections it will appear, that he conceived that it acted in

the solution of the food like a simple diluent, like water heated to the same degree. But facts without number related above shew, that it does not act in this manner, but as a real solvent. That the solution is more speedy and effectual than that obtained by mere water, appears from experiments equally numerous. Moreover, this fluid does not dissolve soft and yielding substances only, but the hardest and most tenacious, contrary to Boerhaave's opinion.

With respect to trituration, the attentive reader will easily anticipate my answer. However remarkable the effects produced by the mechanical action of muscular stomachs may be, intermediate and membranous stomachs have no such power. I have made particular observations on the stomach of the dog, which so nearly resembles that of Man, and it never appeared to have any motion sufficient to break down the food. This was not only proved by thin tubes receiving no injury, but by inspection of the stomach during the time of digestion (CXCIX, CC). The reader will find similar proofs taken from the effects produced by my own stomach, in the CCIXth and CCXth paragraphs. These direct arguments shew the insufficiency of the Boerhaavian hypothesis. It is besides easy to shew its falsity, by examining the foundation on which it rests. He deduces the triturating power from the action of the muscular coat and the contiguous parts; but this coat is so thin in membranous stomachs, that its effects must needs be inconsiderable. Nor is the pressure of the adjacent parts of much importance, at least in the cat and dog;

for upon opening the abdomen and feeling the stomach, I perceived nothing but the pulsation of the arteries, as I had before done in some birds with muscular stomachs (XXXVIII). But this pulsation does not compress the stomach. I likewise perceived by my touch, that this viscus is affected by the vibrations of the neighbouring arteries; but the effects of these vibrations are not more considerable than the pulsation of its own arteries. The whole stomach was lifted up, and depressed by the motion attending respiration. The peristaltic movement was also general in some cases; but the former did not produce contraction, and the contraction produced by the latter was gentle, and incapable of triturating the aliment. It could only agitate, and thus dispose it to be more readily dissolved.

CCXXI. Heat, I readily agree with Boerhaave, in considering as a co-operating cause. My experiments prove its great importance. Though the gastric fluid is not inflammable (LXXXI, CXXIII, CXLIX, CLXXXV, CCXVI), yet it is disposed by warmth to insinuate itself into digestible substances, and reduce them to that gelatinous matter which serves immediately for nutriment. The same observation is applicable to menstrua in general.

I willingly admit, that particles of air, while they are extricated from the food among which they are entangled by means of the saliva, contribute to its more speedy solution.

But I cannot so readily allow, that digestion is promoted by the nervous fluid flowing copiously into the stomach; for its very exist-

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ence is uncertain, and the hypothesis is altogether without foundation.

Much less less can I grant, that the remains of the aliment serve any such purpose as he ascribes to them. The great Haller justly observes, that our appetite and digestion are good only when the stomach is empty (*a*). I have had several opportunities of seeing this confirmed. When I fed a crow, a heron, or a falcon sparingly, the stomach would be empty in six or seven hours; when they would take food again very greedily and digest it completely, as I found upon opening the stomach.

Whether an incipient fermentation contributes to digestion, according to the opinion of this writer, is a question which shall be examined at some length in the following dissertation, as it has been the subject of many modern experiments.

Lastly, I must again differ from him with respect to fibres of flesh, membrane, tendon, cartilage, bone, which, in his opinion, are not digested in the human stomach, but only have their juices expressed; for the experiments I made on myself prove, that the solid parts are really dissolved, if we except only the hardest bones (CCV, CCVIII, CCXII, CCXIII, CCXIV). As Boerhaave endeavoured to reconcile the various opinions of physicians concerning digestion, he seems inclined to adopt in some measure the notion of those who suppose, that the office of the stomach consists in extracting the juice of animal and vegetable matters, among whom

(*a*) *Phys. T. 6.*

Mr. Hecquet has particularly distinguished himself. And a note to this passage, in which he observes, that the stems of hay are still visible in the dung of the horse and the ox, notwithstanding it is chewed so often by the latter, still more clearly explains his idea. I considered it as of great importance to enquire, whether the same thing happens in animals belonging to other classes, which was really the case in some. We have seen, that the two species of crow above-mentioned are both granivorous and carnivorous. I sometimes fed them with wheat a little bruised, and notwithstanding they seemed to eat it greedily, their excrements consisted of dry fragments of this grain. This likewise happened when they had eaten tough flesh. If I put the excrement in water and shook it briskly, the greater part would be suspended, but a little would fall to the bottom; this, upon examination, proved to be cellular substance with a few muscular fibres, of which the particles cohered pretty firmly; the longest pieces measured about an inch. What remained suspended in the water was more than twice as much as that which fell to the bottom, and still retained the characters of flesh. Young crows, which digest more speedily than the adult (LXIX), do not completely digest tough meat. I could easily find cellular substance among their excrement; but when instead of hard they were fed with tender flesh, and with some soft vegetable instead of wheat, the excrement did not shew the least appearance of this sort.

CCXXII. I made the same observation upon frogs. As these animals generally feed

upon insects, I often found among the excrement, when treated in the way just described, legs, thighs, and wings of locusts, and the crustaceous parts of other insects.

Leuwenhoeck, upon examining the excrement of the melvel, found, that it consisted of filaments resembling the hairs of the beard cut off by the razor; these filaments he supposed to be the undigested remains of the fishes which the melvel had eaten (*a*). I can easily believe this to have been the case, especially as it coincides with an observation of my own upon the excrement of the tench, in which, though I could not perceive any fleshy fibres, yet the fragments of bone were distinctly visible. I must however add, that though I have examined the fœculent matter of many other fishes with glasses of various magnifying powers, I could never distinguish the least atom that had the characters of animal or vegetable matter. I have observed the same thing in that of nocturnal and diurnal birds of prey. The tough flesh, of which a small part was voided along with the excrement of crows (CCXXI), is entirely digested by the eagle, falcon, and owl. This observation may be extended to a multitude of birds of various kinds, of which, though I have preserved the names in my journal, to avoid prolixity I will not now enumerate them. Serpents, though so slow of digestion, dissolve their food so completely, that not a vestige of any organized matter appears

(*a*) *Philos. Transact.* n. 152. 1683.

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in their excrements. This at least I have seen in vipers, water and land-snakes.

Upon comparing my observations upon excrement with those related by Boerhaave and others, I think it must be concluded, that considering animals in general, some substances of both kingdoms are voided unchanged along with their excrements, because the gastric fluid is incapable of dissolving them; but others are voided unchanged, only because they do not continue long enough in the stomach to be digested. This is fully proved by my experiments on flesh, membrane, tendon, and bone, the very substances of which Boerhaave supposed, that the solid parts were indigestible. Flesh taken spontaneously by crows, part of which is voided undigested, but when kept many hours in the stomach is completely dissolved, furnishes another decisive proof of the same proposition.

It is surely not necessary to add, that I do not wish by these strictures to lessen the high reputation of the Dutch Hippocrates. Unprovided with experiments of his own, he collected the opinions of others, and framed a system concerning digestion so ingenious and plausible, that I willingly own that I formerly adopted it, and would not now reject it, if I was not compelled by conclusive experiments.

CCXXIII. I will conclude this dissertation, with some remarks on a problem closely connected with researches concerning the efficient cause of digestion. Mr. Hunter, one of the best English anatomists of the present age, frequently in opening dead bodies,
found

found the great curvature of the stomach considerably eroded, and sometimes entirely dissolved. In the former case, the edges of the wound were as soft as half-digested food, and the contents of the stomach had got into the cavity of the abdomen. The author observes, that such a wound cannot have existed in life, having no connection with the disease, and more frequently appearing in persons who have died violent deaths. In order to discover the cause of this phenomenon, he examined the stomachs of various animals, both immediately and some time after death. He observed in several the same appearance. Hence he thought he was enabled to assign the cause. He supposes the solution to be owing to a continuance of digestion after death, and that the gastric fluid is capable of dissolving the stomach when it has now lost its vital principle. From this discovery he infers, that digestion neither depends on the action of the stomach nor on heat, but on the gastric juices, which he considers as the true menstruum of the food (*a*).

CCXXIV. When Mr. Hunter's short but sensible paper came to my hands, I was engaged in experiments on digestion. I had discovered the primary importance of the gastric fluid in this process, and that it acts out of the body; that is to say, in the dead body. I knew also, that after death this fluid issues from the coats of the stomach. From these data I had little difficulty in believing the fact related by the English anatomist, and adopting his explanation of it: nevertheless it was

(*a*) Ph. Trans.

proper to repeat the experiment. Being unprovided with human subjects, I had recourse to animals. Some were opened sooner, and others later after death; but among the numbers I inspected, not one had its great curvature dissolved, or much eroded. I say, much eroded, because I have often seen a little erosion, especially in different fishes, in which, when I had cleared the stomach of its contents, the internal coat was wanting. The injury was always confined to the inferior part of the stomach. If these facts are favourable to Mr. Hunter, a great number are against him. They do not however destroy his observations; mine are only negative, his are positive; and we know that a thousand of the former do not destroy a single one of the latter, provided it is well ascertained. I have no reason to distrust Mr. Hunter, for his paper has that air of ingenuousness and candour which usually accompanies truth.

CCXXV. The ill success of my experiments did not induce me to abandon the idea of digestion after death, it only led me to consider it in another point of view. If it be true, said I to myself, that the gastric fluid exerts its action after death, it must produce some solution of the food. Let then an animal be fed and immediately killed, after some time let it be opened, and let us see whether the food has been at all digested. I determined to bring this obvious inference to the test of experiment; I therefore kept a raven fasting seven hours in order to empty its stomach, and then set before it an hundred and fourteen grains of beef, which were immediately

diately eaten, and must have passed into the stomach, as this bird has no crop. I then killed it, and as it was winter, put it into a stove, where it was left six hours. Supposing this to be a sufficient time for the gastric fluid to exert its action, I opened the stomach, and found the flesh in the following state. It was impregnated with gastric fluid, and was become tender; the colour was changed to a pale red, and the surface had a bitter taste, while the internal parts retained the taste of flesh. After the gastric fluid was wiped away, it weighed only fifty-two grains; it had therefore lost above half its weight in six hours, or, what amounts to the same thing, was above half digested. The pylorus, and the duodenum for about an inch, were occupied by an ash-coloured mucus, which must have been the dissolved part of the flesh.

At the same time I gave another raven, that had in like manner been kept fasting seven hours, an equal quantity of flesh, and killed it in two hours and a quarter. My view was to observe the difference between what had lain six hours in the dead, and two and a quarter in the living stomach, and it was very great; for in this latter case the flesh was totally dissolved, except a few membranous pellicles, which I have found to be always longer in being digested than the muscular fibres; the mucus was the same as before, only in larger quantity, and occupied more of the duodenum. These two experiments compared together prove two things, first, that digestion continues after death; and secondly, that it is then far less considerable than in the living

living animal, though in the present instance the heat of the stove, which was about 100° (a), must have promoted it not a little. The heat of the living raven did not exceed 30° (b).

CCXXVI. Another dead raven was kept five hours in the same stove, after I had forced two dead lampreys, weighing together an hundred and twelve grains, down its throat. One lay in the œsophagus, the other had reached the stomach and was completely decomposed, while the other was indeed entire, but soft and flaccid. This accident proves, that the gastric fluid is capable of producing a sensible degree of digestion at a time when the œsophageal juices are inert.

CCXXVII. These experiments were made in winter. I determined to repeat them the next summer, because then I could expose the dead animals to a greater heat. Accordingly in that season some bruised veal was given to two ravens, which were immediately killed, and left seven hours in a window exposed to the sun. We have already seen in several passages, the influence of heat in promoting artificial digestion (CXLII, CLXXXVI, CCI, CCXVII). Nor did it now appear less considerable. Each raven had eaten sixty-eight grains of flesh, of which there was not an atom left entire; it was all dissolved into the usual gelatinous pulp, and the greater part had passed through the pylorus.

(a) Two hundred fifty-seven deg. Fahr. Ther.

(b) One hundred nine and an half ditto.

These facts, I think, decisively prove, that animals, at least the species just mentioned, continue to digest after death. If we consider the matter rigorously, it will be proper to obviate a difficulty that may be started. However careful we are to kill the animal immediately after it has swallowed food, it is certain, that there will be a short interval between the time the food gets into the stomach and the death of the animal, and that the gastric fluids act upon it during this interval. Moreover, after death they will act for some time just as in life, since the vital heat is not instantly extinguished. The digestion therefore observed in dead animals may, if not entirely, at least in part, be produced by the gastric fluid acting during life, and a short time after death.

Nothing could be more easy than to ascertain the justness of this suspicion, since we have only to thrust a little food into the stomach of a dead and cold animal, and observe the consequence. I made the experiment upon a raven that had been dead an hour, and had now only the temperature of the atmosphere. Forty-two grains of beef cut into pieces were forced into the stomach, which was opened after the bird had lain seven hours exposed to the sun. And here instead of pieces of solid flesh, I found only the usual pulpy mass, partly in the stomach and partly in the duodenum. The solution was therefore effected by the gastric fluid, independently of the powers of life.

CCXXVIII. The experiment was repeated upon an owl and a blackbird, which were killed immediately after meat had been given them,

them, and left seven hours in a warm temperature. The flesh given to the blackbird had been cut into three pieces, which together amounted to eighty-two grains; the owl had swallowed half an ounce and six grains in one piece. Upon opening the stomachs, I found the four pieces; but the surface was covered with a stratum of mucus, which shewed, that the flesh had been dissolved.

I thought, that perhaps if the flesh had remained a longer time in the stomach it would be more digested; but this did not happen, at least when I repeated the two preceding experiments under the same circumstances, except that the birds were exposed to the sun for twenty-two hours, I could not perceive, that the solution of the flesh was carried any further. The entrails emitted a putrid smell, but this was not the case either with the stomach or its contents.

CCXXIX. That I might be warranted in deducing general consequences, I resolved to repeat this singular experiment upon various classes of animals, and therefore had recourse to fishes and quadrupeds. Of the former, the fish-market at Pavia only affords the pike, carp, barbel, tench, eel, and the like; but I took care to procure such as were very fresh. I introduced into the stomach various animal substances, as little fishes, bits of veal and beef, frogs, grubs, &c. and opened them after an interval, sometimes shorter sometimes longer. I will give in a few words what is set down at great length in my Journals. The part of these substances that lay in the œsophagus, a position which they often had,

was unaltered; this was sometimes the case with that which had got into the stomach, but it was generally more or less eroded. A circumstance respecting frogs deserves to be mentioned. The tough skin of these animals was often destroyed, especially at the bottom of the stomach; and where it still remained it was so much softened, that the slightest force was sufficient to lacerate it. Hence it appears, that the gastric fluid of fishes retains its property of dissolving flesh, but in an inferior degree to that of birds, since it did not dissolve so much.

CCXXX. The quadrupeds upon which I made these experiments, were dogs and cats. After keeping them fasting many hours, I gave them a certain quantity of flesh, and then strangled them without delay. Of three dogs and three cats, two of the former and as many of the latter were exposed to the sun for nine hours; the others were left in the shade. In the first the surface of the flesh was gelatinous as usual, but in the last this appearance was scarce perceptible. These experiments confirm the utility, I should rather say, the necessity of heat to digestion in many animals.

CCXXXI. To conclude this curious enquiry, I resolved to see what change would take place upon flesh when the stomach was taken out of the body. I made this experiment upon a cat, a raven, and an owl. Having fed them sparingly, I cut out the stomach, and threw ligatures round the cardia and pylorus to prevent the contents from getting out. They were exposed to the sun in
a vessel

a vessel of water, lest the heat should dry them. In five hours and an half they were opened: the water had transfused through the coats; the surface of the flesh was a little gelatinous, especially in the stomach of the raven and owl; but the solution was trifling, in comparison with that which took place when the stomach was left in the body. This was what might be expected, when the œsophagus no longer poured its liquor into the stomach.

In these experiments I did not perceive any erosion of the stomach, any more than in those made with the view of verifying Mr. Hunter's (CCXXIV). I only saw what I had seen before (*ibid*), a slight excoriation of the inferior part. We must therefore infer, that the coats of the stomach suffer less after death than flesh introduced into it. I set before an hungry dog some pieces cut out of the stomach of another dog; he ate them without hesitation, and was killed immediately. After the body had lain in a warm situation nine hours, the stomach was opened. The pieces were sensibly dissolved, but no change was produced upon the stomach of the animal, if we except the large curvature, which was so much macerated, that the villous coat might easily be rubbed off. It is, I think, not difficult to assign the reason, why the stomachs of dead animals are not liable, like their contents, to be dissolved. These bodies are invested on all sides by the gastric fluid, whereas it acts only on the internal surface of the stomach.

Upon reviewing the experiments related in the CCXXVth and following paragraphs it

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cannot, I think, be doubted, that digestion goes on for some time after death. I therefore entirely agree so far with the celebrated English anatomist, but I cannot with him suppose, that this function is independent of heat (CCXXIII); numberless facts related in this work fully prove the contray.

DISSERTATION VI.

WHETHER THE FOOD FERMENTS IN THE
STOMACH.

CCXXXII. **I** WILL now, agreeably to my promise in the foregoing dissertation (CCXXI), enquire whether the food ferments in the stomach. This opinion was almost universally adopted by physicians about the middle of the last century, an æra at which the explanation of the various functions of the human body was sought in fermentations of various kinds; as it had before been in a subtil matter, as it has since been in electricity, and is at present in divers sorts of elastic fluids. This notion was afterwards combated among others by Boerhaave, who found, by direct observations, that this multiplicity of fermentations did not exist in nature, but was merely the suggestion of fancy. Of the numberless modifications of this process, which physiologists had imagined, he admitted only that very limited and imperfect one, which, according to him, takes place in the stomach. The

food

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food in the stomach of animals, and particularly of Man, is, in his opinion, in circumstances highly favourable to fermentation. The saliva and the gastric fluid serve instead of water; the free access of air, the closeness and heat of the stomach, the nature of the food itself necessarily produce fermentation, as is farther evident from the eructations consequent upon taking food, and the rumbling noise frequently heard in the belly. But the short continuance of the food in that viscus, and other causes, prevent the process from being ever carried to its utmost pitch.

CCXXXIII. Thus far only, according to Boerhaave and his followers, does the fermentation of the food proceed in the stomach. This limitation has been thought too great by Dr. Pringle and Dr. Macbride, two celebrated modern physicians. They find no difficulty in supposing, that a complete fermentation takes place in digestion, and that it is the chief agent in this important function. In their researches on this subject, they have endeavoured to imitate the operations of nature out of the body. They took various animal and vegetable substances, such as are used every day for food; they placed them both by themselves, and mixed with several other substances in a warm temperature, adding a quantity of water or saliva. Under these circumstances they found, that they sooner or later began to ferment; that this process afterwards ran very high, then abated, and at last ended in the decomposition of the several substances, which acquired also a sweet taste. These different gradations of fermentation were evident from the

the swelling, rarefaction, and intestine movement of the mass, from the generation of a multitude of air-bubbles, and from the substances which at first sunk to the bottom, at length floating on the surface of the fluid. These experiments first made by Pringle, and afterwards repeated and varied by Macbride, determined them both to consider digestion as a process merely fermentative. Their theory is as follows. The food divided by mastication and penetrated by the saliva, begins as soon as it gets into the stomach to be agitated by that intestine movement which always accompanies fermentation; this movement is excited by the warmth of the place, by old remains of food, by the gastric fluid, and above all by the saliva, which is particularly adapted to produce and promote this process. The first effect of the intestine commotion will be to raise the solid parts of the aliment to the surface of the gastric liquor; here they will be sustained for some time by the air-bubbles; but on their cessation they will fall down again and be thoroughly incorporated with the fluids of the stomach. The peristaltic motion, the alternate pressure of the diaphragm and abdominal muscles, and the continual pulsation of the adjacent large vessels will render this mixture still more complete. In such a state the food passes into the small intestines, where the fermentative motion produces still greater changes in consequence of the mixture of the bile and pancreatic juice. And now the various kinds of food are changed into a sweet, mild, nutritious matter, which ferments briskly, and is denominated *chyle*. In conformity

formity with this theory, these physicians establish a new system of great importance, according to them, in the practice of medicine. It is ingeniously explained by Pringle, in his Appendix containing Experiments on septic and antiseptic Substances, and by Macbride in his experimental Essays on the Fermentation of alimentary Mixtures.

CCXXXIV. The opinion of these two modern writers have been adopted by many physiologists, while others have still adhered to the doctrine of Boerhaave, concerning an incipient and incomplete fermentation only taking place in the stomach; so that on this subject the physicians of Europe seem to be divided into two sects. When I read Pringle and Macbride, I had only made a few experiments on the digestion of some animal and vegetable substances enclosed in tubes by gallinaceous birds (XXXIX, XL, XLI, XLII, XLIII); and I began to perceive, that the gastric juice acted as a menstruum upon the food. But I could not learn from these experiments, whether fermentation takes place at the time they are dissolved. As indeed the gastric fluid is a solvent, it may act independently of fermentation; chemistry affords numerous instances, in which there is no token of fermentation during the decomposition of the solvend. But there is no absurdity in supposing, that an intestine fermentative motion is generated in the mixture, at the time the gastric fluid dissolves the aliment. And in this case, fermentation would accompany digestion, though it would not according to the doctrine of Pringle and Macbride (CCXXXIII), be the efficient cause.

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In order to obtain information concerning this phænomenon which I had not noticed, I had recourse to further experiments. As the theory in question is entirely founded on the fermentation of animal and vegetable matters in vessels, I set in glass phials bread, flesh, and saliva; bread, flesh, and water; flour, saliva, and flesh; for in these mixtures the writers above-mentioned observed the most rapid fermentation. The phials were stopped, and set in a place where the heat amounted to 20° — 24° (a). The mixtures began, some sooner and others later, to emit air-bubbles, which soon increased in frequency and size; the surface of the liquor was covered with froth, which continued as long as the air was separated. During this time the mass swelled greatly, the intestine commotion was manifest, and the substances immersed being made specifically lighter by the air-bubbles that adhered to them and the increase of bulk, rose to the surface of the fluid. Here then the tokens of fermentation were apparent, and so far I entirely agree with Pringle and Macbride.

CCXXXV. But sound logic forbade me to allow so readily, that the same process takes place in the stomach. I had indeed many reasons for withholding my assent. Not to mention the short continuance of the food in that viscus, a circumstance which did not escape Boerhaave (CCXXXII), I considered, that although the saliva produces and promotes fermentation, the gastric fluid may not

(a) Seventy-seven and eighty-six deg. of Fah. Ther.

have this property. Though the gastric fluid consists in part of saliva, yet as there are several other ingredients, a compound must be formed with properties different from those of its constituent parts. I have adduced many instances to prove, that the gastric fluid retains in some measure its solvent power out of the body; but the saliva never exhibited any such property. I have already shewn, and shall still more clearly shew in the sequel, that flesh immersed in the gastric fluid is not liable to putrefaction; but when put into saliva, it putrefies sooner than in water. This was one of my motives for not immediately adopting the ideas of Pringle and Macbride. It were to be wished, that these physicians had made trial of the gastric liquor also, before they concluded, that what they observed in vessels takes place likewise in the stomach; nor can I well conceive, how they both came to overlook a circumstance of so great importance. Moreover we know, that rest is necessary to fermentation; but the stomach, besides the motion of the whole body, has a movement peculiar to itself. Lastly, should fermentation once begin, it must in all likelihood be soon stopped by the fresh saliva and gastric liquor that are running perpetually, and in no small quantity, into the stomach. These two last objections have been already started, though nobody, as far as I know, has taken the pains of verifying them by experiment. But as the question could be decided in this way only, I determined to undertake to supply the omission.

CCXXXVI. I have already spoken of artificial digestion in several passages. Experiments

ments of this kind afforded me an excellent opportunity of observing, whether the solution of flesh out of the body was accompanied by fermentation, and I never failed in a single instance to attend to this circumstance. I found, that when the vessels remained at rest, a few small air-bubbles began to arise in the space of a few hours; they afterwards became larger and more frequent, and adhering to immersed substances, caused them to rise to the surface of the liquor. This air was either entangled in the mixture, or, according to Pringle and Macbride, formed part of it, was extricated, and rendered elastic by the heat, or what seems more probable, came from both these sources. The mixtures either sunk again or continued to float, while they were dissolved by the gastric menstruum; not the slightest intestine motion was ever perceptible, just contrary to what happens when saliva is employed. If I now and then shook the vessel a few hours after making the infusion, very few air-bubbles were generated, and the mixture hardly ever rose to the surface, though it was just as well dissolved as when the vessel remained at perfect rest. I find in my Journals, that I agitated the vessels upon fourteen several occasions without observing the smallest difference in the result of the experiment. I could not therefore allow, that fermentation was the efficient cause of these artificial digestions, nor even that it was a concomitant circumstance, or an effect; and fresh experiments inclined me more and more to reject this opinion. I have already mentioned the great abundance of gastric fluid in crows, and
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the facility with which they digest their food, more especially nestlings (LXIX, LXXXIII). Among the various trials I made with this fluid out of the body, I endeavoured to renew it, as it is renewed in the stomach. Several glass tubes were filled with it to a certain height, and suspended in a vertical position; into the upper extremity a small funnel was put; some gastric fluid was poured into it from time to time, the narrowness of the orifice of the funnel allowed it to fall only drop by drop into the tubes. The lower extremity of the tubes was not closely stopped, that nearly as much might run out below as fell in from above. Matters being thus arranged, I immersed in the tube flesh and bread, both by themselves and mixed together. The solution was exceedingly speedy, on account both of the warmth of the atmosphere, and the constant renewal of the gastric fluid. Notwithstanding the tubes remained at perfect rest, only a few air-bubbles were discharged; not the least intestine motion could be perceived; the flesh and bread fell immediately to the bottom, and remained there till they were gradually incorporated with the gastric fluid; in short, they were digested without a single circumstance occurring that usually attends fermentation.

CCXXXVII. If this process does not take place out of the body, it seems highly improbable that it should within; however, to be certain of this, it was proper to consult the senses. Is digestion, according to Pringle and Macbride, a fermentative process? Let us then inspect it while it is going on, by
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surprizing Nature in her operation, and seeing in what it consists. With this view, I gave four hens that had been kept fasting twelve hours, some wheat, and in five hours opened the gizzard without killing them. This method I practised in the following experiments, being apprehensive lest opening the animal after death might not answer the end I had in view. Both gizzards were full of grains of wheat mostly broken, and mixed with a semifluid farinaceous paste. The orifice of the pylorus and great part of the duodenum was full of the same paste, which had not in this case much fluidity. Upon examining this paste, both with my naked eye and the microscope, I could not perceive any sign of fermentation; the parts were at perfect rest, and entirely free from air-bubbles. I waited three hours longer before I opened the gizzards of the other two ducks, in order to see whether what had not taken place at the beginning of the process, might not have happened when it was further advanced. In this case, the paste was more diluted with gastric liquor, and of the grains of wheat little was left but the bran; I observed no more intestine motion or air-bubbles than before.

CCXXXVIII. My next experiment was made upon three ravens that had not yet quitted the nest. Two hours after I had fed them with beef, I opened the stomach of one of them. The flesh was half dissolved, but I could not perceive any sign of fermentation. I made the same observation upon the two others, which were opened an hour and three quarters afterwards, notwithstanding

standing the digestion was complete; for nothing remained in the stomach but a dense grey fluid, consisting of flesh dissolved in the gastric fluid.

Of animals with membranous stomachs I examined an owl, several dogs, cats, and land and water-snakes, endeavouring always to make my observations at three distinct times, at the beginning, towards the middle, and at the end of digestion. But at no time did I perceive any tendency to fermentation. In one dog and one cat only did I observe a few air-bubbles among the food after it was completely digested; but there was not the least intestine motion perceptible. Serpents, animals so slow of digestion, were well adapted to shew the progress of this function; but neither did they form an exception to the general observation. These facts obliged me to reject the opinion of the British physicians and their followers; nor do I know whether that of Boerhaave is admissible, who, while he excludes a complete, infers an incipient fermentation, from the eructations that arise after taking food (CCXXXII); but this may be occasioned by the rarefaction of the air entangled among the aliment, by the mere heat of the stomach.

CCXXXIX. Modern chemists have distinguished three species or degrees of fermentation, the vinous or the sweet, the acetous, and the putrid. As they essentially consist in an intestine motion excited by heat and a proper degree of moisture (*a*), and as

(*a*) Macquer Dict. Art. *Fermentation*.

no such motion can be seen in the food in the stomach, it follows, that not even the vinous, much less the acetous or putrid takes place in digestion. It remains to be enquired, whether this function is connected with a principle of acidity, as some suppose, or of putrefaction, according to others. I shall state the facts which appear to favour each of these opinions. In behalf of the first, its advocates adduce acid eructations and vomitings from the human stomach, that disagreeable acid smell which is exhaled from the stomachs of granivorous birds and ruminating animals, the acetous taste of the internal coat; the diminution of the bulk of the contents of the stomach of man and animals; not to mention other arguments that may be seen in modern physiologists, and especially in Haller.

CCXL. The prodigious number of stomachs I have opened, have afforded me opportunities of acquiring full information on this point. In animals strictly carnivorous, such as birds of prey and serpents, the food never appears acetous, either to the taste or smell during the time of digestion. The same observation will apply to frogs and fishes; and it may be extended to omnivorous animals, such as crows, when they feed upon flesh; but the pulstaceous mass resulting from vegetables, and in particular from bread, now and then acquires a slight acidity. I have found the same taste in two dogs, and more frequently in herbivorous animals, such as sheep and oxen; and also in those which are at once herbivorous and granivorous, viz. in the gallinaceous kind; and in the last-men-

tioned class, not only the food in the stomach had an acetous taste, but that in the craw likewise. In the third dissertation will be found some instances of this (CXXXIX, CXL, CXLI, CXLIH). With respect to Man, I will relate what has happened to myself. During the whole month of May and great part of June, I eat strawberries with sugar and white wine at dinner and supper. From this agreeable mixture I never experience any inconvenience in the day-time; but by what I eat in the evening my sleep is frequently disturbed; the contents of my stomach rise almost into my mouth, and then fall back again, leaving a most disgusting sour taste behind. This unpleasant circumstance does not, however, prevent me from recovering my rest, and digesting my food perfectly. I have besides many times been subject to a like disagreeable sensation, after eating too much fruit in summer and autumn. Every man must some time or other have been sensible of his meat and drink having turned sour.

CCXLI. I was further desirous of knowing, whether the acid principle sometimes found in the stomach, is capable of dissolving calcareous earth, and such other bodies as acids act upon. I accordingly gave some carnivorous birds pieces of coral and sea-shells, and they were thrown up without any change of colour, or diminution of weight. This was what might be expected. I next made the same experiment on a hen and a turkey, which were killed in two days. The coral and shells were very much corroded, and the former was reduced to pieces; but a mo-
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ment's reflection shewed me, that the corrosion might be owing to the action of the stomach, and not to an acid. The doubt, however, might easily be removed, by enclosing the same substances in strong metallic tubes. The result of several experiments made in this manner was, first, that the pieces of coral and shell were almost always diminished; but the diminution scarce ever exceeded three or four grains: secondly, that the surface was softened; and thirdly, that it was turned black, especially in the coral. I immersed at the same time the same substances in diluted vinegar, and as similar effects were produced, and particularly the black colour, I inferred, that the phænomena rose from a like cause; lastly, I repeated the experiment on myself. The tubes were covered as before (CCVIII), to prevent any feculent matter from getting into them. They were all voided without inconvenience. When I ate flesh, with the addition only of a little bread, the substances contained were neither diminished nor altered in their colour. But upon eating a large quantity of different vegetables, the coral and shells were generally diminished and darkened. These facts prove the presence of an acid principle in the stomachs of some animals, and man himself. It is, however, not perpetual, but depends on the quality of the food.

CCXLII. This acid soon disappears. I gave several gallinaceous fowls some bread at the same time. The stomachs were examined at different intervals, viz. two, three, three and an half, four, and five hours afterwards. As long as the bread preserved its consistence,

it was frequently acid; but as soon as it was reduced to chyme this taste was totally lost. Nor could I perceive the least sign of it in that which had passed into the duodenum. I made upon myself the following observation.

When the unpleasant acid taste mentioned above (CCXL) came into my mouth in consequence of eating strawberries, I kept myself awake twice during the remainder of the night. Acid eructations continued to arise for some time; they at last ceased; yet from a sense of weight I knew, that the contents of the stomach were not entirely digested; but the flatulence that came from them had no longer the slightest acidity.

CCXLIII. But what produces this acidity in the stomach? Does it come from the gastric fluid, or from the food? There are good grounds for rejecting the former, and admitting the latter of these origins. In the first place, this acidity does not appear on all occasions; I never could observe it arising from flesh. Now if it came from the gastric fluid, why should it not be communicated to every kind of food, since every kind is alike impregnated with it? Secondly, when I ate vegetables, the effects of an acid in my stomach were apparent, but not when I ate flesh (CCXLI). Thirdly, when vegetable food is completely dissolved by the gastric fluid, it then loses all acidity (CCXLII). Lastly, if acid bread be enclosed in tubes and given to a crow, when it is thrown up four or five hours afterwards, the little that remains instead of being sour, is now turned sweet.

CCXLIV.

CCXLIV. Notwithstanding these proofs, that this acidity is not owing to the gastric fluid, but to the tendency the food itself has to turn sour whenever it is in a warm temperature, is it not supposed, that this fluid both in Man and animals is of an acid nature? Have not most of the antient and many modern physicians subscribed to this opinion? I should therefore have incurred the reproach of negligence, if I had not undertaken a chemical analysis of it. The gastric fluid of every animal mentioned in these dissertations, not excepting my own, was submitted to the following experiments. Having taken the precautions above described (LXXXI, CCXV), to procure it in a state of purity, I dropped it upon salt of tartar *per deliquium*, and into the nitrous and marine acids, without ever perceiving any change of colour, any motion or effervescence; whence I was obliged to infer, that the gastric fluid is neither acid nor alkaline, but neutral. I thought it would also be proper to subject those kinds which could be procured in large quantity, as that of the crow, to the action of fire; I therefore entreated my illustrious colleague and friend, Counsellor Scopoli, to undertake the analysis, as he was not only provided with the proper apparatus, but eminent for his skill in chemistry, of which science he is deservedly public professor. He complied with my request, and in a few days favoured me with the following account.

Chemical Analysis of the Gastric Fluid of the Crow.

“The liquor is turbid, and of a darkish colour. When shaken it emits a smell rather disagreeable.

When triturated with quick-lime or salt of tartar, a fetid urinous odour is exhaled.

It does not effervesce with either of the mineral acids. It gives rather a green hue to syrup of violets.

Two drachms exposed to a gentle heat left a dark-coloured sediment weighing two grains, which attracted the humidity of the air. This residuum had a nauseous smell. It did not effervesce with acids.

I next filtered and distilled it. A darkish matter was left upon the filter, which, when it was dried, appeared in the form of a nut-brown powder, of a salt and bitter taste.

The liquor which passed into the receiver was divided into five portions. The first had a slight taste, and an empyreumatic smell. The second had a stronger taste and smell. The third, fourth, and fifth resembled the second, but the last had the strongest empyreuma.

The belly of the retort was almost entirely covered with a white saline substance, which upon being triturated with quick-lime emitted a fetid urinous smell. In the bottom there remained a tough dark-coloured substance, resembling an extract. It did not effervesce with acids; its smell was empyreumatic,

matic, and its taste salt, bitter, and nauseous. This salt is neither acid nor alkaline, for it does not effervesce either with acids or alkalies; but when a little oil of tartar *per deliquium* is mixed with it, it emits a penetrating urinous odour, exactly like that of sal ammoniac.

From these experiments we may conclude, that the gastric fluid contains, first, a pure water; secondly, a saponaceous and gelatinous animal substance; thirdly, sal ammoniac; fourthly, an earthy matter like that which exists in all animal fluids.

The saponaceous substance altered by fire emits that unpleasant empyreumatic smell.

The sal ammoniac being enveloped by the soapy matter does not sublime, as it does when not entangled by other substances.

The gastric fluid of the crow precipitates silver from nitrous acid, and forms luna cornea. This phenomenon might induce us to suppose, that common salt exists in the gastric fluid; but as the salt contained in this fluid is not common salt but sal ammoniac, we must suppose, that the silver is separated from the nitrous, on account of its stronger attraction for the marine acid, which also far exceeds the attraction of the volatile alkali for the latter acid.

I wish you would repeat these observations on the gastric fluid of animals feeding only on vegetables. If in this also sal ammoniac should be found, we must conclude, that the marine acid is generated by the animal powers; and we might suspect, that the marine acid of sea salt is produced by the ani-

mals that inhabit the ocean. This is however a mere conjecture.

I am, &c.

SCOPOLI."

A little after I had received this account from my celebrated colleague, I quitted Pavia, to spend the summer vacation in my own country, where I had no opportunity of making experiments on the gastric fluid of any animal strictly herbivorous, though I earnestly wished for it. I obtained, however, satisfactory proofs from the raven, that the ammoniacal salt does not depend on animal food, but on the powers of life. I fed five ravens for fifteen days on vegetables alone, and then by means of sponges procured a quantity of gastric fluid, which I supposed would have no properties that could be ascribed to animal food. When I made with it the experiments described above, it did not appear to be acid or alkaline; it had a salt taste, and upon pouring a few drops into a solution of silver in the nitrous acid, luna cornea was precipitated. There is therefore every reason to suppose, that if this fluid was distilled, sal ammoniac would be obtained; and therefore, that the marine acid is the product of the animal powers. But whatever we are to think either of this or the other ingenious conjecture of my colleague, which have indeed little connection with our present enquiry, it is certain, both
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from his and my experiments, that the gastric fluid is not either acid or alkaline, but neutral.

CCXLV. But I must not conceal those arguments which are adduced to prove, that there is a latent acid in this fluid, though it cannot be detected by any of the ordinary chemical means. It is well known, that a small quantity of acid will curdle milk, an effect produced in the stomach of animals, and of sucking calves in particular, in which case we cannot suspect any vegetable acid to be present; the phenomenon must, therefore, be attributed to the latent acidity of the gastric fluid. And as it is continually secreted by the internal coat of the stomach, we cannot be surprized, that this coat in some animals should retain the property of curdling milk. This is well known to cooks, who, when they have no rennet, take the innermost coat of the stomach of a fowl and steep it in water; which water, when thus impregnated with the juice of the stomach, will serve for turning milk as well as rennet itself.

Hence some have supposed, that the stomach contains a latent acid. My first step was to ascertain the fact. I therefore triturated the internal coat of a hen with water, which was thus rendered turbid, and in an hour and a half curdled a quantity of milk. The same effect was produced by the internal coat of other gallinaceous birds, viz. the capon, turkey, duck, goose, pigeon, partridge, quail, treated in the same manner. I further discovered, that this property belongs also to intermediate and membranous stomachs, by experi-

experiments on that of the crow, heron, birds of prey, the rabbit, the dog, cat, various reptiles, and several scaly fishes. In these trials the stomachs were fresh. I next tried dried ones, chiefly taken from the gallinaceous class, which being almost of the consistence of horn, become dry in a very short space, and at the same time exceedingly brittle. The results were the same as before. Nor did it make any difference, if they had been kept ever so long. I have had for three years the internal coats of the stomach of several fowls, and upon triturating them with water, while I am writing, they curdle milk as well as at first. If they are pounded and mixed with milk, they answer the purpose equally well.

CCXLVI. But is this property confined to the internal coat? It was easy to determine this by treating the others in the same manner. The nervous coat has this property in some degree, but falls far short of the internal. Whether cut into small pieces and macerated in water, or mixed immediately with milk, the effect is not so speedily produced, nor so considerable, nor are the curds so hard. The muscular and cellular coats have not this property in the smallest degree, at least in gallinaceous birds, upon which these experiments were made. Hence it would seem, that it resides in the internal coat solely; for the effects produced by the nervous coat, may be owing to its lying in contact with the former.

CCXLVII. But is this property inherent in the internal coat, or adventitious, and owing to the
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the gastric fluid with which it is impregnated? I incline to the latter opinion, since the gastric fluid so readily curdles milk. I should weary my reader, was I to recount all my experiments. I will therefore only say, that the gastric fluid, from whatever animal it was obtained, possesses this property, whether I procured it by sponges, by opening the stomach and expressing it out of the glands, and the mouths of the little arteries, with which this viscus in general abounds. I have further found, that the gastric fluid need not be fresh. That of crows, at least, preserves its virtue for three months.

CCXLVIII. But is it a necessary consequence of these experiments, that the gastric fluid contains an acid? As no chemical test shews this quality, there can be no just motive to admit it, unless it can be proved to be a necessary consequence of the curdling of milk. This is maintained by the illustrious Macquer among others, who is of opinion, that whatever bodies of the animal and vegetable kingdom coagulate milk, have either a manifest or occult acidity (a).

The foundation of this opinion is the common observation, that acids are the sole cause of the curdling of milk. To this reasoning, I shall only oppose a single fact: I have discovered, that though several animal substances are incapable of producing this effect, yet others have this property. Thus for instance, if the blood or bile of a turkey be mixed with milk, it will retain its fluidity; but

(a) Art. Milk.

pieces of the heart, liver, or lungs of that bird, will curdle it readily. This observation is not merely owing to accident; I have made the experiment repeatedly with different turkeys, and always with the same success. If therefore the coagulation of milk be always owing to an acid, we must suppose an acid in the heart, liver, and lungs of the turkey. I am not ignorant, that many chemists, in opposition to the Boerhavian school, think a real acid exists in the different parts of animals, and particularly in the blood; but according to this hypothesis, I cannot comprehend, why the blood of the turkey and other animals does not coagulate milk. With respect to the latent acid of the gastric fluid, I shall very willingly leave my readers to adopt what opinion they shall think most probable. The milk I employed for my experiments, was sometimes that of the sheep, but generally of the cow. It curdles spontaneously, as every one knows, sooner or later, according to the temperature of the atmosphere. When I mixed it with gastric juice, or any other fluid, I always left another portion by itself. In the former case, the coagulation soon took place without any sign of acidity, whereas milk alone required several hours, and sometimes a day or two, and the coagulum had always an acid taste.

CCXLIX. But it is time to consider the reasons adduced by others, to prove that digestion is attended with an incipient putrefaction. These reasons are founded upon facts related by different authors, and detailed in their order

order by Haller, in his great work (*a*). Nothing, according to them, can be more evident, than the signs of putrefaction during digestion. The stomach of a hyena and of a serpent, have been observed to emit an intolerable stench. The breath of the lion and eagle is very fœtid, as also that of the dog, when digestion has been prevented by the exhibition of opium. A dog without taking opium, was observed to admit an odour of excrement from his stomach; the food in the stomach of birds has nearly this smell. The same observation has been applied to fishes; and the instance of a dog-fish has been adduced, of which the stomach was full of a fœtid jelly, that contained the food dissolved. The contents of the human stomach sometimes become fœtid. Vegetable substances also degenerate into a putrid mass, when they continue long in the stomach, as appears from the putrid smell they exhale, the green colour they impart to tincture of mallows, and the alkaline principle they afford on distillation.

After having related these facts, the Swiss physiologist proceeds to give his own opinion. He thinks, that in digestion there is only an incipient, not a complete putrefaction; which only takes place when the food remains a long time in the stomach, as is evident from the facts just mentioned. He also supposes, that the change produced by the digestive powers, especially in the human stomach, approaches nearer to putrefaction than acciden-

cy; this he infers, from the putrid smell that exhales from flesh found in the stomach of some animals, notwithstanding there has been no impediment to digestion (*a*). This opinion, adopted before Haller by Boerhaave (*b*), has moreover been received by two celebrated writers, Gardana (*c*), and Macquer (*d*).

CCL. Notwithstanding the respectable authority of these writers, I do not think the facts adduced sufficient to persuade an impartial philosopher; they are not only too few, but were observed by mere accident; nor had the observers the smallest intention of entering into a full discussion of this point.

Though the time requisite for digestion in different animals is different, yet in many it does not exceed five or six hours, and in some is still shorter. Now it seemed proper to examine what change flesh set to putrify, would undergo in that space of time; I therefore took some fresh veal cut into small pieces, and put it in a phial of water, which was stoppered with paper. The phial was put into a stove, where the mercury rose to between 30 and 35°.

About the beginning of the fourth hour, the flesh had lost its red colour, and was turning blue. It was also become flabby, but for nine hours it had no putrid smell. Mutton and beef, in several trials, did not answer to

(*a*) L. c.

(*b*) Chem. T. 2.

(*c*) Essai pour servir à l'Histoire de la Putrefaction.

(*d*) Art. common Salt.

this time exactly, but no bad smell was ever perceptible for eight hours. These experiments shew, that flesh eaten by many animals, among which Man may be enumerated, has not time to run into the putrefactive fermentation, especially as the temperature of animals is lower than that to which these several sorts of meat were exposed. However, for greater certainty, I made the following trials. I have before mentioned introducing into the stomachs of crows, pyriform glass vessels, of which the small end was open, and came out at the mouth (LXXXIX).

I now took two of them, and putting some beef, with a little water, into one, and some veal into the other, forced them down the throat of the crows. In order to examine the state of the flesh, I now and then drew them up, and immediately returned them. Between the ninth and tenth hours, the beef emitted an odour, which though it could not be called putrid, was disagreeable. At the expiration of the tenth hour, there was a distinct putrid smell that became gradually stronger and stronger. In a day the flesh turned livid, acquired a nauseous taste, and the particles began to separate. The same appearances took place rather sooner in the veal. It therefore appears, that flesh in the heat of this species of bird requires a longer time to putrify, than to be digested. After the glass vessels were taken out of the stomach, I gave one of them the same quantity of beef and veal; and upon opening the stomach in three hours, found that it was entirely consumed.

CCLI. These experiments prove, that no putrid tendency is ever acquired by meat during digestion. Nor did I ever perceive any such tendency in food lying in the stomach (LXV, CCIX); yet as I had never made experiments for this express purpose, and as some physiologists adduce facts to prove the contrary (CCXLIX), I was under the necessity of examining the stomach of various animals, with this sole view.

Four hens were fed with kid, and in two hours one was killed: the stomach was full; the flesh still retained its natural sweet flavour, which at the surface was mixed with a bitterish taste, occasioned by its being impregnated with the gastric fluid. It had no smell, except that of this fluid. An hour afterwards the stomach of another hen was examined; and here the flesh was beginning to be converted into a gelatinous paste, its smell was rather disagreeable; I know not how to describe it, but it was not at all penetrating, or putrid; the colour was still reddish, it had not the least nauseous taste, nor did it effervesce with acids, or change the colour of syrup of violets. Thus we see, it shewed no sign either of incipient, or advanced putrefaction. In another hour the third hen was killed: the stomach contained a pultaceous mass, more fluid than in the former case; but there was not the smallest token of putrefaction, any more than in the fourth hen, which was opened three hours afterwards, when the craw was empty, and the contents of the gizzard were now dissolved.

CCLII.

CCLII. Some frogs just killed were set before two herons; the birds being hungry, devoured them greedily. In six hours one of them was opened; but whether the toughness of the skin retarded digestion, or whether that process is slow in herons, the frogs had not lost their shape; though the heads and limbs were either separated, or on the point of being separated from the trunk, and the flesh was become very soft. The taste, except the usual bitterness, had nothing nauseous, and the smell was by no means putrid. I waited five hours longer before I killed the second, when I found but little flesh in the stomach, and that little was entirely decomposed, but did not emit the least putrid smell.

If fowls and herons afforded no token of putrefaction, much less could I expect it from the birds upon which my next trials were made; I mean, young owls, which digest flesh in three, or at most four hours. A young dog and cat were next fed, at the same time, with boiled beef. The former was opened in four hours and an half. The stomach was full of a mass of softened flesh, which emitted a very slight smell, exactly resembling the smell of the gastric fluid. The stomach of the cat was opened in five hours and an half, and was found to contain some remains of flesh, or rather a pulpy matter, which as in the former case, had the smell of gastric fluid. The flesh, when nearly digested, did not change the colour of syrup of violets, or effervesce with acids.

CCLIII. There are animals which retain the food in the stomach for a much longer
 Vol. I. T time,

time, as the falcon. Of that upon which I made so many experiments, I have already observed, that it would devour a whole pigeon at once, and continue without food the whole day afterwards (CLX). Whence, as also from its great bulk, we may infer, that the flesh remains a long time in the body before it is entirely digested. Some months after this was killed, another of a different species fell into my hands; it was of a larger size, and had no craw, so that the food passed immediately into the stomach. Notwithstanding it was pretty tame, and therefore valuable, yet I sacrificed it for the sake of these experiments, eighteen hours after it had devoured a chicken. What remained in the stomach weighed two ounces; it consisted of a pulp, in which the fibres could yet be discerned; but neither when subjected to the before-mentioned chemical trials, or when smelled and tasted, did it shew any sign of putrefaction. * But among the animals that retain their food for a long time in the stomach, those with cold blood, and especially vipers, are, as we have seen, the most remarkable. A piece of lizard's tail preserved somewhat of its muscular structure, after having remained five days in the stomach of a land snake (CXVIII). Three water snakes had not consumed all their food at the end of three days (CXXI). Another not even in six days (CXXV). A lizard remained sixteen days in the stomach of a viper, without losing its natural form (CXXVII). Other cold animals, such as eels, newts, and frogs, must not be forgotten. Four eels that had eaten fish, retained a little after the expiration

ration of three days, eighteen hours (CXXIX). On the fifth day, some frogs had not quite digested pieces of intestine (CVI); which also happened to newts, two days after they had been fed with earth-worms (CVIII). But notwithstanding the food continued so long in the stomach of these several animals, I have expressly noticed, that it had begun to putrify (CXXVII).

CCLIV. I have met only with two instances which do not coincide with this invariable constancy of nature, though they do not detract from the certainty of the consequences that are to be deduced from it. Among the crows that were obliged to swallow tubes for a considerable length of time, some suffered in their health, and became poor, though they were copiously supplied with food. But as they did not take it voluntarily, and as it was my wish to keep them alive for the sake of my experiments, I forced some flesh down the throats of two, but to no purpose; for they both died, one thirteen, the other fifteen hours afterwards. My curiosity led me to open them, and I found that the flesh continued whole and undigested, and moreover that it was become putrid. But this evidently arose from the morbid condition of the animal, by which the gastric fluid was altered, and rendered inefficacious. For this species of bird, as I have seen in a hundred instances, digests flesh very speedily, and without any token of putrefaction appearing. It is also probable, that the putrid state of the food in the animals mentioned in the CCXLIXth paragraph, arose from their morbid condition;

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tion ; especially as it remained so long in the stomach of some of them. It may also happen, that an animal may die without any disease preceding, and yet the food be found fœtid, if the stomach is opened after a long interval, as frequently happens.

In the same paragraph, the breath of the lion and eagle are said to be fœtid. That of the lion I never had an opportunity of examining, but with the eagle it was far otherwise ; for when I stroaked the head gently, it would sometimes open its mouth, and raise a little cry ; on these occasions it necessarily made a long expiration, and in winter the breath appeared in the form of a little cloud. This cloud I have often smelled, and caused others to smell, both when the bird was fasting, and when the stomach was full, and when the food was recently digested ; but it was never fœtid, and indeed did not seem to have any kind of odour.

CCLV. The experiments described in the CCL, CCLI, CCLII, and CCLIII^d paragraphs, not only shew that digestion is unaccompanied by putrefaction, but might induce us to suppose, that the stomach is provided with an antiseptic principle. Flesh inclosed in the pyriform glasses that were introduced into the stomach of crows, shewed clear signs of putrefaction in ten hours ; whereas in eighteen they shew no appearances of the kind, when it is in immediate contact with the stomach (CCLIII). And although serpents, and the other amphibious animals above-mentioned (CCLIII) are of a cold temperature, yet in their temperature, which is nearly equal to that of the atmosphere, flesh

flesh becomes putrid in two days, and sometimes in one, and sometimes even in a shorter time, while in their stomachs it remains untainted frequently for a much longer space. I could not therefore but conclude, that there is present in these cases, some cause that prevents the corruption which supervenes out of the body. What then is this cause? It was not difficult to detect it. I called to mind those unfinished digestions, which take place when flesh is immersed in gastric fluid contained in phials. Where it is dissolved without ever turning putrid, notwithstanding it is kept long enough, and exposed to a sufficient heat. I could not then doubt, that the gastric juices are at once the solvent, and the preservative from putrefaction. Further reflection furnished me with proofs still more decisive. It appears from various passages in the preceding dissertations, that in attempts to produce artificial digestion, little or no solution takes place, unless the fluid extracted from the stomach is exposed to a brisk heat (CXLII, CLXXXVI, CCI, CCXVII). But without this condition, it retains its antiseptic powers (CLXXXVI, CCXVII). Two phials, containing one some gastric fluid from a crow, and the other from a dog, together with some veal and mutton, were kept thirty-seven days in winter, in an apartment without fire: the flesh was not either consumed or turned putrid; while some that was immersed in water, began to emit a fœtid smell on the seventh, and about the thirtieth day was changed into a very offensive liquamen. It is proper to add, that the gastric fluid at last loses, though kept in phials ever so closely

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stopped, its antiseptic quality, but it never becomes putrid itself. This at least I found to be the case with some taken from a crow, after it had been kept two months.

CCLVI. The discovery of this antiseptic property led me to enquire, what would be the effect of immersing flesh, more or less putrid, in gastric fluid. Four portions that had an insupportable smell, were set in four bottles, which I filled with four different kinds of gastric fluid, viz. of a dog, a crow, an owl, and an eagle. This was done in March, and the bottles were kept twenty-five days in an apartment, where the heat was never less than 8, and never exceeded 12°. I could not perceive that it was at all more dissolved than if it had been immersed in water. With respect to the foetid smell in the phials containing lamb and veal, it continued unchanged; but in the two others, which contained fowl and pigeon, it seemed rather diminished. This result suggested to me, that the gastric fluid might not only impede putrefaction, but restore putrified substances. I therefore repeated the experiment in June, and found that my suspicion was well founded. Some fowl and pigeon, in which putrefaction was pretty far advanced, were immersed in the gastric fluid of a dog and falcon, and remained in it thirty-seven hours, in which time they were reduced to a jelly, but had lost most of their disgusting smell. On comparing this with the preceding experiment, I conjectured, that the superior efficacy of the gastric fluid in the latter case, proceeded from the warmth of the season: and this induced me to expose the same flesh under the same circum-

circumstances, to the sun about the middle of June. And now ten hours completely took away the fœtid smell. I did not neglect making the same experiment with the gastric fluid of other animals; the flesh generally lost its disagreeable odour, but sometimes for a reason, which I cannot assign, retained it in part. It is proper to add, that the recent fluid was always more efficacious than the old.

CCLVII. If we consider the CCLV and CCLVIth paragraphs, we must conclude, that putrid flesh loses this quality in the stomach of animals. Before I attempted to ascertain this point by experiment, nature herself gave me a decisive proof of it. At the time I kept a great number of fowls for my enquiries, I perceived that when they are allowed to eat at will, they cram their craw so full, that it is sixteen or twenty hours before it is completely evacuated. Curiosity led me to kill a cockrel that had about an ounce of meat, which happened to be bruised flesh, remaining in its craw: and I was struck with surprize, when I perceived that it had a strong putrid smell: it was become soft, had a dull red colour, and a nauseous taste: I immediately proceeded to examine the contents of the stomach; but here I found the flesh quite decomposed with a bitter sweet taste, and a smell by no means fœtid. The liquor therefore of the stomach, had corrected the putrid quality which the flesh had acquired in the craw. The same thing took place in some hens. The flesh in the craw became putrid in sixteen hours, while that in the stomach had no disagreeable odour. It should, however, be remarked,

that the putrefactive fermentation never runs so high within the craw, as it does out of the body, even when the heat is less strong. Whence I suspected, that the fluid of the craw might also possess an antiseptic power, though in a degree very far inferior to that of the stomach.

CCLVIII. I took a putrid piece of beef's lights, and dividing it into five portions, fastened a string to each, and then thrust them into the stomachs of five ravens. The end of the string was brought out at the beak, as on former occasions (LXVIII), that I might be able to examine the flesh at pleasure. In three quarters of an hour, two of the pieces were drawn up: they were wasted, and at first seemed to have lost their putrid smell, but upon wiping off the gastric fluid, it became again sensible, but it was much diminished. Half an hour afterwards another piece, upon examination, was found to be still more wasted, and to have lost almost all its bad odour, even when the gastric fluid was carefully wiped away. In an hour afterwards the two remaining pieces were drawn up. They were reduced to the size of a pea, and it would have been impossible to tell that they had been ever putrid, so perfectly were they recovered; even the taste had nothing disgusting, except the bitterness which is always present on such occasions.

The great length of the neck prevented me from repeating this experiment upon the heron. I forced a semi-putrid frog, from which the skin had been taken, into the stomach, but I could not draw it up again; it was therefore necessary to cut the string at
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the beak, when the bird immediately swallowed it. It was my intention to kill the heron in about an hour, that I might examine what change the frog had undergone. But it was vomited before that time, probably on account of its being a disgusting food; for however greedily the heron devours living fishes and frogs, it abstains from them when they are turning putrid. The gastric fluid had, notwithstanding, exerted both its antiseptic and solvent powers, during the forty-three minutes the frog had been in the stomach. Some tin tubes were then filled with putrid fish, and given to the animal; they were not, as before, thrown up, perhaps, because the putrid matter was not in contact with the stomach. The bird was killed three hours afterwards; what remained in the tubes weighed one-seventh of an ounce; it resembled a thick gelatinous paste, in which a few fleshy fibres might yet be distinguished, and which retained no vestige of its former putrid state.

CCLIX. I treated several small birds of prey, such as the two species of owl above described, and a young hawk, as I had done crows (CLVIII). They were fed with intestine, liver, and lungs of sheep, more or less putrid. Solution took place, and the putrefaction was corrected according to the time of the continuance of the flesh in the stomach. The hawk twice threw up what it had swallowed, probably, because the putrid state of it disagreed with the stomach, for this never happened when it was fed with fresh meat. The gastric juices of the eagle produced the same effect upon flesh inclosed in tubes, and intro-

introduced into its stomach. Animals of cold blood having very slow digestive powers, were long in correcting the putrefaction of flesh. This effect, however, was at last produced. The only precaution necessary, was to return the substances into the stomach when they were vomited, which often happened.

The last experiments I made with this view, were upon a cat, a dog, and myself. I was obliged to force the putrid flesh down the throat of these animals, for notwithstanding they were exceedingly hungry, they obstinately refused it. The dog retained what was forced upon him, but the cat vomited it along with a quantity of foam, and a liquor that appeared to be gastric fluid. The flesh, which when it was given was exceedingly foetid, had now lost its smell entirely; of this, another cat, by eating it without afterwards throwing it up, gave me a clear proof. Upon opening the stomach, I found the flesh half digested, and with no other smell than that which fresh meat usually emits in like circumstances. In two hours and a half the dog was opened. The flesh lay in a little lake of gastric fluid, nearly decomposed, nor did it either in taste or smell resemble tainted meat. The experiment I made on myself, consisted in swallowing, at five different times, five tubes covered with linen, like those mentioned in the CCVIIIth paragraph: they were full of different sorts of putrid flesh. I voided them separately, and in each there was some of the contents remaining, but not one exhibited the smallest token of putrefaction. Hence then it appears, that the various classes
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of animals, and Man among the rest, in an healthy state, are endowed with the power, not only of checking the putrefaction of substances lodged in the stomach, but also of correcting them when already putrid.

CCLX. By this discovery I was led to reflect, that many animals living upon flesh, and matters that have a tendency to run into the putrefactive fermentation, never feed but upon such as are fresh and sweet; and that, if by any accident putrid food should get into the stomach, they are subject to vomiting and various bad symptoms, and even death itself: some instances of vomiting excited by this cause, may be seen above (CCLVIII, CCLIX); while on the other hand, many animals delight in corrupted substances, as the multitude of loathsome insects and worms that reside in sewers and sepulchres, and feed upon decaying carcases. Among birds and quadrupeds, there are also some that seek tainted flesh; such are the crow, the kite, the vulture, among the former; and among the latter, the chacal and the hyena. While other animals fly the miasmata that arise from bodies in such a state, these seek and are guided by them to their abominable repasts. But now we are acquainted with the antiseptic virtue of the gastric fluid, the disgusting manners of these animals ought no longer to surprize us, for the food, however putrid, must be totally changed before it is converted into nutriment and animalized. And although the putrid quality is corrected by other animals, yet food in that state is noxious to them, on account of the disagreeable impression it makes on the organs of smell and taste, as
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also upon the stomach, by which, and particularly by their noisome miasmata, the nervous system is probably irritated. It besides seems likely, that the antiseptic power of the gastric fluid of the former, is greater and more efficacious, and consequently that it more readily and more completely corrects putrefaction. Habit, which is justly reputed a second nature, may bring animals, that naturally abominate putrid food, to live very well upon it. We have already witnessed the conversion of a pigeon from a granivorous into a carnivorous animal (CLXXV); and I brought it to eat not only fresh flesh, but such as was foetid, and even completely putrified. The bird at first absolutely refused it, and I was obliged to force it into the stomach; for some days it suffered from this treatment, and became evidently leaner. But by degrees nature became inured to the food, and the pigeon, stimulated by hunger, took it spontaneously, till at last it recovered its plumpness; and now its appetite for tainted, was as keen as it had been before for sweet meat. We may learn from this instance, that custom is capable of changing disagreeable, and even noxious food, into good nourishment.

CCLXI. But what shall we suppose enables the gastric fluid to check and correct putrefaction? As it contains a salt, and that of the ammonical kind (CCXLVI), and as besides the experiments of Pringle shew, that all salts, whether acid, alkaline, or neutral, whether volatile or fixed, are antiseptic (*a*), it is ob-

(*a*) Appendix, containing experiments on septic and antiseptic substances.

obvious to conjecture, that these two qualities arise from the same source. I conceived, however, before I determined absolutely, that it would be proper to attempt a few experiments. It is observed by Pringle, that we must employ common salt, which so nearly resembles sal ammoniac, in considerable quantity, if we wish it to act as an antiseptic; otherwise, it is so far from checking, that it promotes putrefaction. Thus a drachm of salt, dissolved in two ounces of water, keeps meat sweet but a little while, and twenty-five grains a still less time; while ten, fifteen, and even twenty grains hasten its corruption. This paradox has been confirmed in France by the learned Mr. Gardane. Notwithstanding these authorities, I determined to bring it to the test of experiment. I therefore took four phials, and putting into each three penny-weights, six grains of fresh beef, pounded very small, I poured upon it an ounce and half of water. In the first phial were dissolved ten grains of common salt, in the second fifteen, in the third twenty, and the fourth was left without salt, as a term of comparison. The temperature of the place where they were kept, was about fifteen degrees. The first phial began first to emit a foetid smell, the fourth next, then the second, and lastly the third. The other tokens of putrefaction appeared in the same order. When sal ammoniac was substituted in the place of common salt, the only difference in the result, consisted in the phial which contained no salt, and that which contained ten grains, beginning to exhale a putrid smell at the same time. It appears, therefore, that Pringle's experiment

periment was accurate, and that the same thing nearly is true of sal ammoniac. In order to determine whether the antiseptic property of the gastric fluid arises from the sal ammoniac it contains, I dissolved a quantity of that salt by degrees in water, till it had nearly acquired the same saltiness as the gastric fluid; some bruised flesh was then immersed in it. That the water and the liquor of the stomach had nearly the same saltiness, I assured myself, both by tasting it, and by dropping a few drops of each into a solution of silver in the nitrous acid, when each afforded the same white precipitate. But it is this cause that prevents putrefaction; for the flesh immersed in the salt water, emitted a foetid odour sooner than other flesh of the same kind, infused in common water: and although when more sal ammoniac was employed, putrefaction was retarded, it was not prevented; to attain this end, eighteen or twenty times as much salt as is contained in the gastric fluid was requisite. These facts seem clearly to shew, that the antiseptic quality of the gastric fluid does not depend on the small quantity of sal ammoniac it contains.

CCLXII. From the septic power of common salt in small quantity, Mr. Gardane deduces a consequence, which it may be proper to notice in passing. He thinks the common salt we take with our food, being always in little doses, forwards digestion, by promoting putrefaction; upon which, according to him, as we have seen above (CCXLIX), that function depends. Though my numerous experiments completely destroy this supposition, yet it seemed worth while to try
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what would happen to flesh seasoned with such a proportion of common salt as hastens putrefaction, and given to different animals. Some tubes, filled with flesh thus prepared, and others, with some of the same kind, without salt, were given to a dog and a cat. The animals were opened in five hours, and upon examining the tubes, I could not perceive that the salt had occasioned any difference. What remained undissolved, had still a slight salt taste, but not the least disagreeable smell; and it was just as much wasted as the other. It therefore appears, that this small dose of salt had neither promoted digestion, nor produced any tendency to putrefaction, being overpowered by the antiseptic quality of the gastric fluid.

CCLXIII. But to return from this digestion. If the salt contained in the gastric fluid is not the cause of its antiseptic power, to what other principle can it be owing? Macbride's theory concerning the origin of this property in so many bodies, has great ingenuity. The cohesion and solidity of substances, is in his opinion owing to the fixed air they contain. Now when by any means this is taken away, the mutual adhesion of the several parts will be destroyed, and the body will either run into the putrefactive fermentation, or crumble into dust, according to the nature of its constituent parts. Hence it necessarily follows, that whatever substance has the power of impeding the separation of fixed air, or restoring it when separated, will also prevent or correct putrefaction. But antiseptic matters have, according to this physician, such a power. A piece of flesh, for instance,

instance, surrounded by a substance of this kind, is kept sweet, because the fixed air cannot make its escape; and that, probably, on account of its pores being blocked up by the finer particles of the antiseptic matter. Hence the flesh will long preserve its natural taste and consistence. If it has already become putrid, it will receive fixed air from the antiseptic body, and hence cease by degrees to exhale a foetid smell, lose its fluidity and flabbiness, and at last recover its sweetness and firmness (a).

Will not this theory account for the antiseptic power of the gastric fluid? Without going out of my way to examine the foundation on which it rests, I will observe, that it seems by no means to afford the information wanted, since the gastric fluid is an antiseptic of a singular sort. Other substances possessing this property, while they keep away putrefaction, preserve or restore the cohesion of the parts; whereas the gastric fluid being at once an antiseptic and solvent, while it prevents or corrects putrefaction, reduces bodies into very small particles. We must therefore conclude, that the property of this animal fluid arises from some other principle, though I cannot determine what that principle is, both for want of experimental data, and on account of the imperfect state in which physicians have left the theory of putrefaction. I therefore chose to acknowledge my ignorance, rather than invent some gratuitous hypothesis; such a mode of proceeding would ill agree with the disposition of one, who has

(a) Macbride, l. c.

no other object in view than the discovery of truth.

CCLXIV. For the sake of my readers, it may be proper to recapitulate what has been proved in this dissertation. First, of the three species of fermentation established by modern chemists and naturalists, viz. the sweet, the acetous, and the putrid, neither takes place in digestion. Secondly, Though this function is sometimes accompanied with an acid, yet this principle disappears entirely towards the conclusion of it. Thirdly, Putrefaction never in health attends digestion. Fourthly, The gastric fluid is a real antiseptic. I suppose my proofs, however conclusive, will not avail with those who establish it as an axiom, that wherever there is heat and moisture, there must be fermentation; and think that it must therefore necessarily take place in the food, and not only in the stomach and intestines, but in the chyloferous and sanguiferous vessels: they indeed limit their doctrine so far as to say, that whereas out of the body it goes on rapidly, and with an intestine commotion, in the body of it is slow, weak, and generally imperceptible. Let me intreat these learned and zealous advocates for fermentation to reflect, that my experiments are not directly repugnant to theirs. I only pretend to shew, that not the smallest sensible fermentation takes place in the stomach of animals or man. With respect to insensible fermentation, as it is amongst uncertain things, sound logic forbids me alike either to admit or reject it.

A P P E N D I X.

ON THE
D I G E S T I O N
OF THE
S T O M A C H AFTER D E A T H.

By JOHN HUNTER, F. R. S. and Surgeon
to ST. GEORGE'S Hospital*.

AN accurate knowledge of the appearances in animal bodies that die of a violent death, that is, in perfect health, or in a sound state, ought to be considered as a necessary foundation for judging of the state of the body in those that are diseased.

But as an animal body undergoes changes after death, or when dead, it has never been sufficiently considered what those changes are; and till this be done, it is impossible we should judge accurately of the appearances in dead bodies. The diseases which the living body undergoes (mortification excepted) are always connected with the living principle, and are not in the least similar to what may be called diseases or changes in the dead body:

* See Philosophical Transactions, Vol. LXII. p. 447.

without this knowledge, our judgment of the appearances in dead bodies must often be very imperfect, or very erroneous; we may see appearances which are natural, and may suppose them to have arisen from disease; we may see diseased parts, and suppose them in a natural state; and we may suppose a circumstance to have existed before death, which was really a consequence of it; or we may imagine it to be a natural change after death, when it was truly a disease of the living body. It is easy to see therefore, how a man in this state of ignorance must blunder, when he comes to connect the appearances in a dead body with the symptoms that were observed in life; and indeed, all the usefulness of opening dead bodies depends upon the judgment and sagacity with which this sort of comparison is made.

There is a case of a mixed nature, which cannot be reckoned a process of the living body, nor of the dead; it participates of both, inasmuch as its cause arises from the living, yet cannot take effect till after death.

This shall be the object of the present paper; and, to render the subject more intelligible, it will be necessary to give some general ideas concerning the cause and effects.

An animal substance, when joined with the living principle, cannot undergo any change in its properties but as an animal; this principle always acting and preserving the substance, which it inhabits, from dissolution, and from being changed according to the natural changes, which other substances, applied to it, undergo.

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There are a great many powers in nature, which the living principle does not enable the animal matter, with which it is combined, to resist, viz. the mechanical and most of the stronger chemical solvents. It renders it however capable of resisting the powers of fermentation, digestion, and perhaps several others, which are well known to act on this same matter, when deprived of the living principle, and entirely to decompose it. The number of powers, which thus act differently on the living and dead animal substance, is not ascertained: we shall take notice of two, which can only affect this substance when deprived of the living principle; which are, putrefaction and digestion. Putrefaction is an effect which arises spontaneously; digestion is an effect of another principle acting upon it, and shall here be considered a little more particularly.

Animals, or parts of animals, possessed of the living principle, when taken into the stomach, are not the least affected by the powers of that viscus, so long as the animal principle remains; hence it is that we find animals of various kinds living in the stomach, or even hatched and bred there: but the moment that any of those lose the living principle, they become subject to the digestive powers of the stomach. If it were possible for a man's hand, for example, to be introduced into the stomach of a living animal, and kept there for some considerable time, it would be found, that the dissolvent powers of the stomach could have no effect upon it; but if the same hand were separated from the body, and introduced into the same stomach,

we should then find that the stomach would immediately act upon it.

Indeed, if this were not the case, we should find that the stomach itself ought to have been made of indigestible materials; for, if the living principle was not capable of preserving animal substances from undergoing that process, the stomach itself would be digested.

But we find on the contrary, that the stomach, which at one instant, that is, while possessed of the living principle, was capable of resisting the digestive powers which it contained, the next moment, viz. when deprived of the living principle, is itself capable of being digested, either by the digestive powers of other stomachs, or by the remains of that power which it had of digesting other things.

From these observations, we are led to account for an appearance which we often find in the stomachs of dead bodies; and at the same time they throw a considerable light upon the nature of digestion. The appearance which has been hinted at, is a dissolution of the stomach at its greatest extremity; in consequence of which, there is frequently a considerable aperture made in that viscus. The edges of this opening appear to be half dissolved, very much like that kind of dissolution which fleshy parts undergo when half digested in a living stomach, or when dissolved by a caustic alkali, viz. pulpy, tender, and ragged.

In these cases, the contents of the stomach are generally found loose in the cavity of the abdomen, about the spleen and diaphragm. In many subjects this digestive power extends

much further than through the stomach. I have often found, that after it had dissolved the stomach at the usual place, the contents of the stomach had come into contact with the spleen and diaphragm, had partly dissolved the adjacent side of the spleen, and had dissolved the diaphragm quite through; so that the contents of the stomach were found in the cavity of the thorax, and had even affected the lungs in a small degree.

There are very few dead bodies, in which the stomach is not, at its great end, in some degree digested; and one who is acquainted with dissections, can easily trace the gradations from the smallest to the greatest.

To be sensible of this effect, nothing more is necessary, than to compare the inner surface of the great end of the stomach, with any other part of the inner surface; what is found, will appear soft, spongy, and granulated, and without distinct blood-vessels, opaque and thick; while the other will appear smooth, thin, and more transparent; and the vessels will be seen ramifying in its substance, and upon squeezing the blood which they contain from the larger branches to the smaller, it will be found to pass out at the digested ends of the vessels, and appear like drops on the inner surface.

These appearances I had often seen, and I do suppose that they had been seen by others; but I was at a loss to account for them; at first, I supposed them to have been produced during life, and was therefore disposed to look upon them as the cause of death; but I never found that they had any connection with the symptoms: and I was still more at

a loss to account for these appearances, when I found that they were most frequent in those who died of violent deaths, which made me suspect that the true cause was not even imagined (*a*).

At this time I was making many experiments upon digestion, on different animals, all of which were killed, at different times, after being fed with different kinds of food; some of them were not opened immediately after death, and in some of them I found the appearances above described in the stomach. For, pursuing the enquiry about digestion, I got the stomachs of a vast variety of fish, which all die of violent deaths, and all may be said to die in perfect health, and with their

(*a*) The first time that I had occasion to observe this appearance in such as died of violence and suddenly, and in whom therefore I could not easily suppose it to be the effect of disease in the living body, was in a man who had his skull fractured, and was killed outright by one blow of a poker. Just before this accident, he had been in perfect health, and had taken a hearty supper of cold meat, cheese, bread, and ale. Upon opening the abdomen, I found that the stomach, though it still contained a good deal, was dissolved at its great end, and a considerable part of these its contents lay loose in the general cavity of the belly. This appearance puzzled me very much. The second time was at St. George's Hospital, in a man who died a few hours after receiving a blow on his head, which fractured his skull likewise. From those two cases, among other conjectures about so strange an appearance, I began to suspect that it might be peculiar to cases of fractured skulls; and therefore, whenever I had an opportunity, I examined the stomach of every person who died of that accident: but I found many of them which had not this appearance. Afterwards I met with it in a soldier who had been hanged.

stomach

stomach commonly full; in these animals we see the progress of digestion most distinctly; for as they swallowed their food whole, that is, without mastication, and swallow fish that are much larger than the digesting part of the stomach can contain (the shape of the fish swallowed being very favourable for this enquiry), we find in many instances that the part of the swallowed fish which is lodged in the digesting part of the stomach is more or less dissolved, while that part which remains in the œsophagus is perfectly sound.

And in many of these I found, that this digesting part of the stomach was itself reduced to the same dissolved state as the digested part of the food.

Being employed upon this subject, and therefore enabled to account more readily for appearances which had any connection with it, and observing that the half-dissolved parts of the stomach, &c. were similar to the half-digested food, it immediately struck me, that it was from the process of digestion going on after death, that the stomach, being dead, was no longer capable of resisting the powers of that menstruum, which itself had formed for the digestion of its contents; with this idea, I set about making experiments to produce these appearances at pleasure, which would have taught us how long the animal ought to live after feeding, and how long it should remain after death before it is opened; and above all, to find out the method of producing the greatest digestive power in the living stomach: but this pursuit led me into an unbounded field.

These

These appearances throw considerable light on the principles of digestion; they shew that it is not mechanical power, nor contractions of the stomach, nor heat, but something secreted in the coats of the stomach, which is thrown into its cavity, and there animalises the food (*a*), or assimilates it to the nature of the blood. The power of this juice is confined or limited to certain substances, especially of the vegetable and animal kingdoms; and although this menstruum is capable of acting independently of the stomach, yet it is obliged to that viscus for its continuance.

(*a*) In all the animals, whether carnivorous or not, upon which I made observations or experiments to discover whether or not there was an acid in the stomach, (and I tried this in a great variety), I constantly found that there was an acid, but not a strong one, in the juices contained in that viscus in a natural state.

EXPERIMENTS
 CONCERNING
 DIGESTION.
 TRANSLATED FROM THE
 INAUGURAL DISSERTATION
 OF
 DR. STEVENS.

Published at Edinburgh in 1777.

THE following experiments were made at Edinburgh upon an Hufar, a man of weak understanding, who gained a miserable livelihood, by swallowing stones for the amusement of the common people, at the imminent hazard of his life. He began this practice at the age of seven, and has now followed it twenty years. His stomach is so much distended, that he can swallow several stones at a time; and these may not only be plainly felt, but may be heard, whenever the hypogastric region is struck.

EXPERI-

E X P E R I M E N T . I.

At eight o'clock in the evening, I gave the subject of my experiments a hollow silver sphere, divided into two cavities by a partition, and perforated on the surface with a great number of holes, capable of admitting a needle: into one of these cavities was put four scruples and a half of raw beef, and into the other five scruples of raw bleak. The sphere was voided in twenty-one hours, when the beef was found to have lost one scruple and a half, and the fish two scruples. The rest was much softened, but had no disagreeable smell.

II. A few days afterwards he took the same sphere, containing in one cavity a scruple and four grains of raw beef, and in the other four scruples and eight grains of the same boiled. In forty-three hours the sphere was returned, and the raw flesh had lost one scruple and two grains, and the boiled one scruple and sixteen grains.

III. Suspecting that if these substances were divided, so that the solvent could have freer access to them, more of them would be dissolved, I procured another sphere with holes, so large as to receive a crow's quill, and enclosed some beef a little masticated in it. It was voided quite empty, thirty-eight hours after it was swallowed.

IV. Seeing how readily the chewed meat was dissolved, I thought of trying whether it would be as soon dissolved in a sphere with large holes, but without being chewed. I therefore put a scruple and eight grains of pork into one cavity, and into the other the
same

same quantity of cheese. The sphere was retained forty-three hours, at the end of which not the smallest remains of either pork or cheese could be found.

V. He afterwards swallowed the same sphere, containing in one partition some roasted turkey, and in the other some boiled salt herring. In forty-six hours it was voided, and nothing of the turkey or herring now appeared, both having been completely dissolved.

VI. Having found that animal substances, though inclosed in tubes, are easily concocted, I next determined to try whether vegetables, which are more difficultly digested, would be so too. I therefore enclosed an equal quantity of raw parsnep and potatoe in a sphere. It was voided after having continued forty-eight hours in the alimentary canal, when both species of vegetable were found to be dissolved.

VII. Pieces of apple and turnep, both raw and boiled, were dissolved in thirty-six hours.

VIII. He next swallowed some grains of wheat, rye, barley, oats, and pease, contained in a sphere, which remained several hours in the alimentary canal, but no alteration was produced on any of its contents, except upon the pease, which were swoln, and burst by the humidity they had imbibed.

IX. The readiness with which the gastric fluid had acted upon roasted animal substances, induced me to try what change would be produced by it upon hard ones, such as bone. I therefore inclosed in one partition of a sphere, some of the bone from a leg of mutton,

ton, and in the other part of a turkey's wing. The sphere was retained forty-eight hours. The bone was weighed, and found to have lost nothing of its weight, while the flesh, skin, and ligaments were quite dissolved, so that the bones of the wing were now quite separate; but they had undergone no perceptible alteration.

X. Inanimate matters being so readily soluble, I resolved to enquire how far living animals are capable of resisting the action of this powerful menstruum. With this view, an animal supposed to be destitute of pores, and, according to my experiments, capable of sustaining a degree of heat equal to the human temperature, was enclosed in a sphere perforated with small holes, to prevent the leech from wounding the stomach. The Hussar took it, and voided it about the usual time, when nothing was found except a black viscid miasma, the remains of the digested leech. This experiment was repeated with earth-worms, and they were dissolved with equal facility. But as they cannot so well support the human temperature, it is probable they died before they began to be dissolved*.

It was my intencion to make more experiments of this kind, but as the Hussar left Edinburgh soon afterwards, I was obliged to have recourse to dogs and ruminating animals.

XI. A whelp, three months old, having been kept fourteen hours without food, was forced to swallow four oval ivory globes, of

* Perhaps this is also the case with leeches.

different sizes, and perforated with many small holes. One contained beef, another haddock, a third potatoe, and a fourth cabbage, all raw, and weighing each sixteen grains. In four hours the animal was killed and opened. The globes were found in the stomach, and their respective contents were diminished in the following proportions: The fish had lost nine grains, the beef five, the potatoe three, and the cabbage one. The globes themselves appeared to be thinner, but as I had no suspicion that the ivory would be affected by the gastric fluid, I did not weigh them before the experiment. I could not therefore exactly ascertain their diminution.

XII. Having procured a whelp five months old, it was kept fasting sixteen hours, and then four of the globes used in the foregoing experiment, each containing a certain quantity of mutton, turbot, parsnep, and potatoe were forced upon it. These substances had been previously exposed to the action of fire, and each weighed sixteen grains. Seven hours afterwards the animal was killed, and the globes were taken out of the stomach; when the fish was found to have lost ten grains and a half, the mutton six, the potatoe five, and the parsnep nothing. The spheres were become still thinner, but I had as before, neglected to weigh them.

XIII. A dog six months old was kept fasting the usual time, and the same four spheres were given him. The first contained sixteen grains of boiled mutton, the second as much boiled fish, the third the same quantity of boiled potatoe, and the fourth of boiled parsnep. In eight hours it was killed and opened.

ed. The globes were found greatly altered (*a*). The extreme parts, not the middle, were totally dissolved, so that the contents lay loose in the stomach. The spheres, before the experiment, weighed together three scruples sixteen grains; the fragments weighed only one scruple and twenty grains. The mutton and fish were entirely concocted, the potatoe had lost twenty-one grains; but the parsnep was unchanged.

XIV. Being surprized at the speedy solution of ivory by the gastric fluid, I determined to subject other hard bodies to its action. I therefore carefully weighed three pieces of a sheep's thigh bone, and gave them to a dog that had been long kept fasting. Seven hours afterwards the animal was killed, and the bones were taken out of the stomach. The first had lost seven, the second nine, and the third twelve grains. The solution began at the internal surface, and advanced towards the center, so that the cavity was considerably augmented (*b*).

(*a*) The author has expressed this very obscurely. *Partes eorum extremæ, non mediæ, ex toto solutæ sunt, &c.* I confess I am yet to learn, what are the extreme parts or ends of a sphere. T.

(*b*) In order to assure myself that this solution was not owing to fermentation, or an acid, I immersed a bone of the same kind, in an alimentary mixture, consisting of roasted beef, wheaten bread and water, beaten into a pulp. When it had remained forty-eight hours in a temperature, equal to 102 deg. of Fahr. Therm. it was examined: the fermentation had run very high, and the acidity was strong, but the bone had undergone no diminution. It was, however, much softened.

I more—

I moreover obliged my dog to swallow pieces of cartilage, but I found that the gastric fluid produced no effect upon them.

XV. As the ivory spheres and bones were so readily dissolved in the foregoing experiments, I was induced to make trial of some bodies still harder. With this view I procured some cylindrical tin tubes, perforated with a great number of holes; of which four were given to a dog that had been kept fasting twelve hours. The first contained sixteen grains of roasted beef, the second the same quantity of veal; the third of fat, and the fourth of wheaten bread. In ten hours the animal was killed and opened, and the tubes were taken out of its stomach. The beef and bread were quite dissolved; the veal had lost only ten grains, and the fat eight and a half. The tubes had not undergone the smallest alteration.

XVI. As in the last experiment the veal was not so soon dissolved as the beef, I began to suspect that the flesh of young animals in general is less easy to digest than that of old ones. I therefore took care to repeat the experiment with lamb and mutton, which were put in equal quantities into two tubes. The result was as before. In seven hours the mutton was quite dissolved, whereas the lamb had lost only ten grains. The remains of veal and lamb in these experiments were surrounded with a viscid gelatinous matter.

XVII. Sixteen grains of raw beef, and the same quantity of roasted were inclosed in two tubes, and given to a dog, which was killed seven hours afterwards, when the former was

found to have lost fifteen grains, while the latter was completely dissolved.

XVIII. The same experiment was repeated with fish instead of flesh. Sixteen grains of raw and as much boiled haddock, were inclosed in two tubes, and given to a dog. When he was killed, no remains of the boiled could be found; the raw portion had lost fourteen grains.

XIX. I next enquired whether quadrupeds or birds are most easily digested. For this purpose, equal quantities of beef, mutton, and fowl were inclosed in three tubes, and given to a dog; they were each roasted, and weighed sixteen grains. Upon killing the dog, and examining the tubes, I found that the mutton and beef had been dissolved, while the fowl had only lost eleven grains.

Most of the experiments related above, were repeated oftener than once, and afforded the same result. We cannot therefore entertain any doubt concerning the mode of digestion in this class of animals. Whether the concoction of ruminating animals is effected in the same manner, I endeavoured to ascertain by the following experiments.

XX. I gave a sheep four cylindrical tin tubes, each containing sixteen grains of raw beef, salmon, turnep, or potatoe; six hours afterwards the animal was killed; the tubes were found in the first stomach. The fish and flesh were unaltered, whereas the turnep and potatoe were quite dissolved.

XXI. The same experiment being repeated with the same substances boiled, afforded the same result. The vegetables were digested, and the beef and salmon unchanged.

XXII.

XXII. Having found that the sheep digests vegetables very readily, but is incapable of dissolving animal substances, I had next recourse to the ox. Four tubes, one containing raw beef, another fish; a third chopped hay, and the fourth leaves of pot-herbs; were given to an animal of this species, and it was killed ten hours afterwards. The tubes lay in the first stomach; the fish and flesh were not altered; but I could find no remains of the hay or herbs.

Many experiments of the same kind were made upon this animal, and they led me to the same conclusion, viz. that the gastric fluid of the ox kind, easily and speedily dissolves vegetables, but is incapable of producing this effect upon animal substances.

In all these experiments, I attribute the solution of the food to a powerful menstruum secreted by the coats of the stomach. It may be objected, that my experiments do not clearly shew whether the food is concocted by the gastric fluid, or by fermentation, for both causes may act equally upon aliment inclosed in the spheres. But besides the arguments already adduced to shew (a), that fermentation does not produce this effect, many circumstances attending these experiments, clearly shew the efficacy of the gastric liquor. For in the experiments in which the food was not quite dissolved, the solution always began at the surface, and proceeded towards the center, and what remained, shewed no tokens of fermentation.

In the XIIIth experiment ivory was dissolved, while parsnep, a vegetable of soft

(a) In the part that has been omitted.

texture, and liable to fermentation, was not at all altered.

To remove every doubt, the following experiment was several times repeated, and always afforded the same result.

XXIII. Having kept a dog fasting eighteen hours, that his stomach might be free from the remains of food, I killed it, and collected about half an ounce of pure gastric fluid, which was put into a phial with twelve grains of roast beef. The same quantity of the same beef was put into another phial, containing water, in order to serve for a term of comparison. Both phials were placed in a furnace, of which the temperature was equal to $102-104^{\circ}$ of Fahrenheit's thermometer. In eight hours the beef in the gastric fluid was quite dissolved, whereas that in the water had undergone no perceptible alteration. In twenty-four hours both phials were taken out of the furnace and carefully examined. The food dissolved in the gastric fluid emitted a rancid and pungent, but by no means a putrid odour; it resembled very much the smell of burnt feathers. The meat in the other phial was quite putrid, and intolerably fetid; but its bulk was not diminished.

I carefully observed the phial containing the gastric fluid during the solution, but could perceive no air-bubbles arising, or any other token of fermentation. I repeated this experiment with masticated meat, when the solution was much more speedily completed.

I afterwards made trial of mutton, veal, lamb, and other animal, together with a great variety

variety of vegetable substances; all were easily dissolved; but the time requisite for the completion of this process was different, and answered exactly to the results of the preceding experiments.

As in this experiment there was no sign of fermentation or putrefaction, I suspected that the gastric fluid, as well as the saliva (*a*), retards both the one and the other. In order to determine this, I made the following experiment.

XXIV. I took two alimentary mixtures, each consisting of mutton and bread in equal quantities. Upon one, half an ounce of the recent gastric juice of a dog was poured, and upon the other the same quantity of pure water. Both mixtures were beaten to a pulp, and inclosed in phials accurately stopped; they were then set in a furnace, heated to the 102nd deg. of Fahrenheit's thermometer. Fermentation took place in a few hours in the phial that contained the water, the solid contents rose to the surface, and air was extricated with a considerable intestine motion. The mixture immersed in the gastric fluid, remained fourteen hours with scarce any tokens of fermentation; but a short time afterwards, this process evidently took place. The bread and flesh arose to the surface of the mixture, a sediment began to be deposited, and air-bubbles were continually extricated. But these phænomena continued much longer than in the other phial; the commotion

(*a*) Where did the author learn that the saliva checks fermentation? It appears to forward it both from the experiments of Pringle, Macbride, Spallanzani and others. T.

was less violent, and the air was not so rapidly extricated. When the fermentation had entirely ceased, the taste of the mixture in this phial was indeed acid, but not so strong as in the other, and it was converted into a fluid by the solvent power of the gastric liquor.

XXV. I divided a piece of putrid mutton into two parts, each of which was put into a separate phial, and to one, half an ounce of the recent gastric fluid of a dog was added, and to the other, which was designed as a term of comparison, as much water. They were set in a cool place, and two days afterwards I examined them, when the latter emitted an intolerably putrid smell, and the other, though it had yet a bad odour, did not smell so disagreeably as the preceding, nor even so disagreeably as at first. Upon shaking the phial, the meat fell to pieces, but it was not quite dissolved. This, perhaps, happened, because it was not exposed to a sufficient heat.

These experiments throw great light on digestion. They shew, that it is not the effect of heat, trituration, putrefaction, or fermentation alone, but of a powerful solvent, secreted by the coats of the stomach, which converts the aliment into a fluid, resembling the blood. If it should be asked, what defends the organ itself, I would answer, that it is the vital principle, as Mr. Hunter's (a) observations shew; after death it is dissolved

(a) Philosoph. Trans. for 1772. The ingenious observer seems, however, to attribute too much to this

dissolved as readily as any other inanimate substance. It is probable, that every species of animal has its peculiar gastric liquor, capable of dissolving certain substances only. Some living solely upon vegetables, others upon animals, and these cannot be obliged to feed upon plants, by a fast of whatever continuance. All, by an infallible instinct, choose what is best adapted to their gastric fluid. The food, when dissolved, is expelled from the stomach, and being mixed with

this principle. He supposes, that whatever possesses it, is capable of resisting the action of the gastric liquor; his arguments by no means prove this. Worms, indeed, live in the human stomach, but it does not follow, that other animals also can, for nature may have given them a particular structure of body. The following considerations will render the general proposition very doubtful. Fishes swallow and digest living crabs, lobsters, &c. The leech is concocted by the human stomach, though it has no pores, and can sustain a temperature equal to that of man. Cornelius found a snake half digested in a bird's stomach, but still alive. Plot saw one eye consumed, while the fish was alive. It seems therefore probable, that the gastric liquor acts also upon living things. Perhaps, likewise, it is sometimes so changed, as to act on the stomach itself. The following cases communicated by Dr. Monro render this probable. A lady, that used to complain of pain in the stomach, died suddenly. Upon opening the body, a hole was found in the left side, and the coats were relaxed as if they were half putrified. There were no appearances of gangrene. A boy died after having long struggled with similar pains. The stomach exhibited the very same appearance, if we except the hole. From the preceding symptoms, one may venture to suppose, that some alteration was produced before death. But this is only conjecture, and future experiments must determine the question.

the bile and pancreatic juice in the duodenum, is changed into a mild blood and inodorous liquid, which is denominated chyle. The chyle is absorbed by numberless vessels, and is carried by the thoracic duct into the subclavian vein, in order to repair the constant waste of the body.

EXPERIMENTS ON DIGESTION,

By Mr. G O S S E,

As they are related by Mr. SENEBIER, in his CONSIDERATIONS on the MODE in which SPALLANZANI conducted his RESEARCHES on the same FUNCTION.

THE experiments of Spallanzani on Digestion so clearly illustrate the subject, as to leave nothing further to be desired respecting the concoction of solid food! And though his experiments on himself put the finishing hand to his researches, they are not however sufficient to satisfy every question that may be asked.—For what happens to the food while it continues in the stomach? When does digestion begin? When does it end? What kind of food is most easy of digestion? To such questions, the event of experiments made with silver tubes swallowed full of meat, and afterwards voided with the excrements, can afford no satisfactory reply. To ascertain these particulars, it was necessary to be able to vomit at will, with-

out injuring the stomach, an advantage really possessed by Mr. Goffe. The experiments of this gentleman were made with the utmost care; and being ignorant of the researches of Spallanzani, and having adopted no system, he observed the phenomena only; but these he observed with the eye of an experienced naturalist, an able chemist, and an ingenuous philosopher, who regarded truth above all things. His inquiries were directed to the benefit of his own health; had he enjoyed greater leisure, he would have directed them to the advancement of science. Mr. Goffe, at my request, sent me an account of his experiments, with permission to insert them in this preface; and to subjoin such observations as I might think proper, he himself not having annexed any.

OBSERVATIONS ON SWALLOWING ATMOSPHERIC AIR.

Mr. Goffe, when a boy, acquired the art of swallowing air; he conceived the idea, in consequence of feeling some acidity in his stomach; the air excited vomiting, which relieved him. He had ever afterwards recourse, in his indigestions, to the same remedy, and it constantly operated without nausea or fatigue to the stomach: by drinking some water, and then throwing it up, he contrived to wash this organ, as well as if he had held it in his hand. Finding himself in possession of such an advantage, he thought of making observations on digestion, which he began to do in 1760.

In

In order to introduce air into his stomach he held his breath, shut his mouth, pressed the air against his palate, and thus obliged it to descend like any other substance, by the exertion of the muscles of deglutition. Its passage along the œsophagus was rendered sensible, both by the space it occupied, and the noise it made.

By this method, in appearance so simple, though not very easy to practise, he excited vomiting at pleasure: he supposes the effect to be produced by the dilatation of the air, which is occasioned by the warmth of the stomach, for the colder it was, the less was necessary. Every mouthful of air was nearly equal to a cubic inch.

Two mouthfuls, at a temperature of 4° or 5° above the freezing point, swallowed when his stomach was empty, occasioned a painful tension, which could only be removed by evacuating the air and taking food. When he wishes to vomit several times in succession, he is obliged to swallow fresh air every time, till nothing but this fluid unmixed comes from the stomach.

We have here a new emetic, so gentle in its operation, that it would be of great use to the sick if they could take it, or if an easy way of administering it could be contrived.

GENERAL EXPERIMENTS ON DIGESTION.

I. Mr. Goffe, being in perfect health, took at dinner soup made of lean salt beef, with common bread of Paris, and chopped herbs, among which were chevril, borage, and
* X onions;

onions; he then eat lean beef with salt, and afterwards spinach boiled in broth, with the same bread baked the day before, and probably fermented with yeast; he drank red wine of Orleans. Half an hour after dinner, he swallowed some air and vomited: he found, that the food, though well masticated, had undergone scarce any change; the several things he had taken retained their taste, and their weight was nearly the same as at first, a small quantity of gastric liquor being only mixed with them.

II. Having made the same dinner another day, he swallowed an equal quantity of air an hour afterwards, and after vomiting, found the food reduced to a pulp, with which there was mixed much gastric fluid. The taste of the aliments was not much changed, except that of the wine, which was now much milder; the gastric juice had produced an addition of weight; but though the food had lain an hour in the stomach, it did not seem to have undergone any degree of fermentation.

III. He repeated the experiment two hours after a dinner of the same kind. The aliments were, as before, reduced to a pultaceous mass, but there was no change of taste, nor any indication of fermentation; he could, however, recover only half of what he had eaten.

CONSEQUENCES OF THESE EXPERIMENTS.

These experiments agree with those of Spallanzani. The food, we see, is not dissolved till it is mixed with gastric liquor: the

the juices of the stomach act with great celerity, for in about an hour and an half they change the food into a pultaceous matter, and render it fluid, without however altering its nature: when digestion is properly carried on, there is no appearance of acidity or alkalescence: the food does not all ferment, and the process of digestion is not compleated, till between two and three hours have elapsed.

The experiments of Mr. Gossé confirm a conjecture I had formed, upon the cause which obliges the gastric fluid to issue from the glands by which it is secreted. I entertained a suspicion, that the weight of the food forces out this liquor, by stretching the internal coat of the stomach, just as the same effect is produced by stretching it with the hand after the death of the animal. In reality, the weight of the food is scarce changed during the first half hour; the tension must be continued longer, in order that there may be an afflux of gastric liquor. Further, when Mr. Gossé drank milk, it required above half an hour to curdle: whence we may conclude, that at first there is no gastric fluid in the stomach, and that the milk presses it out by its weight when it has continued in the stomach some time.

EXPERIMENTS TO DETERMINE THE DEGREE OF DIGESTIBILITY OF DIFFERENT KINDS OF FOOD.

Mr. Gossé having observed what happens in his stomach in the ordinary progress of digestion, wished to know, by means of vomit-
ing,

ing, the respective digestibility of various kinds of food, that he might choose such as would best agree with him. The following is the result of his experiments, as divided by him into three classes.

The first contains such matters as appeared to him not to be digestible. The second, such as were in part digested. The third, those of easy digestion.

I. SUBSTANCES NOT DIGESTIBLE, OR SUCH AS WERE NOT DIGESTED IN THE USUAL TIME.

ANIMAL SUBSTANCES.

I. The tendinous aponeurotic parts of *beef, veal, pork, poultry, skate.*

II. The *bones.*

III. The *oily* or *fatty* substances of these animals.

IV. The *white of an egg* indurated by heat.

VEGETABLE SUBSTANCES.

V. Oily or emulsive seeds, such as walnuts, almonds, nuts, pine-kernels, pistacchio nuts, grape-stones, the seeds of apples, pears, oranges, lemons, olives and cocoa.

VI. Unctuous oils expressed from walnuts, almonds, nuts, and olives.

VII. Dried grapes, well masticated, remained in his stomach for two days.

VIII. The *skin* of fresh grapes.

IX. The skin or rind of farinaceous substances, as pease, beans, lentil, wheat, barley.

X.

X. The pods of pease and kidney beans.

XI. The skin of stone-fruits, as cherries, apricots, plums, peaches, damascenes.

XII. The huik of fruits with grains or seeds, as of apples, pears, oranges, lemons. Conserves of oranges and citrons, notwithstanding the preparation they undergo, are very difficult of digestion.

XIII. The *capsules* of fruits with grains, as of apples and pears.

XIV. Ligneous stones, as those of cherries and plums.

It should be observed, that these seeds do not, any more than the emulfive, lose their vegetative power by lying in the stomach: the germination of some is forwarded by continuing there. How many plants fail to grow, because their seeds are dispersed without manure or sustenance? Bittersweet, (*Solanum dulcamara*) mistletoe, hemp, and other plants, which sometimes grow upon trees, are produced by means of the excrement of birds.

II. SUBSTANCES LESS INDIGESTIBLE, PART OF WHICH MR. GOSSE DIGESTED.

ANIMAL SUBSTANCES.

I. *Pork* dressed in all the different methods.

II. Black-puddings.

III. Yolk of eggs roasted.

IV. Fritters of eggs. The eggs almost constantly acquired an alkaline character, and a taste of liver of sulphur, produced by the alkali

alkali in the white, and the sulphur discovered by Mr. Deyeux in the yolk.

V. Fried eggs and bacon were very difficult of digestion. The fat of the bacon prevented the alkalescence of the eggs, but acidity often ensued.

VEGETABLE SUBSTANCES.

VI. Sallad of raw herbs, as lettuce, dandelion, cresses, succory. The bitterness of some of these herbs seemed to facilitate digestion. The oil and vinegar used for seasoning sallad tend to retard digestion, but it is promoted by the salt and pepper. Mr. Goffe could not long continue the use of raw vegetables, on account of the acidity they produced.

VII. White cabbage seemed less easy of digestion than the red, and the nerves of the leaves, less than the parenchymatous substance.

VIII. Beet, cardoons.

IX. Boiled and raw onions and leeks.

X. Roots of scurvy-grass, red and yellow carrots, and succory, are more difficult to digest in sallad, than in any other way.

XI. The pulp of fruit with seeds, when it is not fluid.

XII. Warm bread, on account of the strong acidity it produces.

XIII. Figs, fresh and dry.

XIV. Sweet pastry occasioned an insupportable acidity.

XV. All these substances became more difficult of digestion, when they were fried in butter or oil.

Mr.

Mr. Goffe further remarks, that these several kinds of food, if they should not be quite dissolved in the stomach, are digested in their passage through the intestines, whether by a continuation of the action of the gastric liquor, or on account of the admixture of the bile, pancreatic juice, &c.

III. SUBSTANCES EASY OF DIGESTION, SUCH AS ARE REDUCED TO A PULP IN AN HOUR, OR AN HOUR AND HALF.

ANIMAL SUBSTANCES.

I. Veal, lamb, and in general the flesh of young animals, is sooner digested than of the old. All kind of *poultry*, and especially such as is young.

II. *Fresh eggs* not hardened by boiling.

III. Cow's milk.

IV. Perch boiled, with a little salt, with parsley. When it is well fried, it does not digest so well, and the same thing happens when it is seasoned with oil, wine and white sauce.

VEGETABLE SUBSTANCES.

V. Herbs, as spinach mixed with sorrel, are less easily digestible. Celery, but the nerves do not yield so readily. The tops of asparagus, hops, and the *ornithogallus* of the Pyrenees, known by the name of mountain hops.

VI. The bottom, or the placenta of artichokes.

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** X

VII.

VII. The boiled pulp of fruits with seeds and stones, seasoned with sugar, becomes more easy of digestion.

VIII. The pulp or meal of farinaceous seeds, as wheat, barley, rice, maize, pease, beans, chesnuts, &c. roasted chesnuts are more difficult of digestion.

IX. Different sorts of wheaten bread, without butter, the second day after baking. The crust seemed to him more digestible than the crumb. The salted bread of Geneva, is sooner digested than the Parisian bread made without salt. Bread of rye and buck-wheat does not digest so well; the same observation may be applied to brown bread, in proportion as it contains more bran.

X. Rapes, turneps, potatoes, parsneps of good quality, and not too old.

XI. Gum Arabic; but the acid of this substance is soon felt. The Arabians, perhaps, among whom it is an article of food, obviate this effect by some preservative.

SUBSTANCES WHICH FACILITATED MR. GOSSE'S DIGESTION.

I. Sea or common salt.

II. Spices, as pepper, cannella, nutmeg, cloves.

III. Mustard, scurvy-grass, horse-radish, radish.

IV. Capers.

V. Wine, spirituous liquors in small quantities.

VI. Cheese, and more especially old cheese.

VII. Sugar.

VIII. Various bitters.

SUB-

SUBSTANCES WHICH RETARDED HIS DIGESTION.

I. Water, particularly when hot, taken in large quantities. It occasions the food to pass into the intestines, without being properly dissolved.

II. All acids.

III. All astringents; twenty-four grains of Peruvian bark, taken half an hour after dinner, stopped digestion.

IV. All unctuous substances.

V. A strong decoction of bitter-sweet prevented, upon one occasion, the digestion of the most digestible substances, and they turned sour.

VI. A grain of kermes, taken after dinner, produced the same effect.

VII. As also did a grain of corrosive sublimate.

Lastly, He observed, that employment, after a meal, suspended or retarded digestion; as likewise did leaning with the breast against a table. He remarked that repose of mind, a vertical position of the body, gentle exercise after taking food, were favourable to digestion.

The benefit which medicine and the health of mankind may derive from such experiments is obvious. It were to be wished, that they should be repeated and varied, by making trial, for instance, of two or three different kinds of fruit at once, &c.

The results of Mr. Goffe's experiments are different from those of Mr. Reuss, as related

related in a Dissertation printed at Edinburgh in 1768. He examined the state of his food three hours after having taken it; but he took an emetic in order to bring it up; now it is obvious, that an emetic disorders the stomach, and that in three hours, food of every kind must undergo too great changes, to shew the respective digestibility of various substances.

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TRANSLATION

OF

ITALIAN, LATIN, &c. PASSAGES,

Page 117. *Come le rane*, &c. As frogs scour along the water, at the approach of the water-snake, without stopping, till they have gained the dry ground.

Page 202. *Receptum est*, &c. It is commonly supposed that animals can digest bone: Boerhaave formerly followed the opinion of Van Helmont. To attain greater certainty, he observed with care what happens to food in the stomach of animals of powerful digestion: he found that bone is not digested.

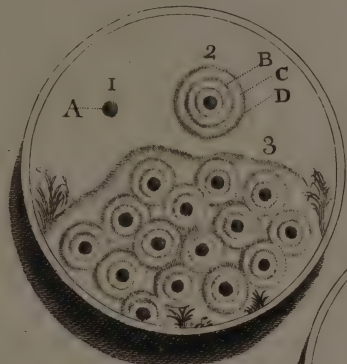
He gave an hungry dog some intestine, which were eagerly devoured, but instead of being concocted, they were partially voided, and hanging out at the anus, tormented the poor animal. He afterwards gave another dog some bones anointed with butter; but nothing was dissolved, except what water will dissolve. Of flesh, the dry and expressed fibres were voided, and ligament, in passing through the alimentary canal, underwent no change.

Page 208. In the excrement of the dog, we find fragments of bone little altered. It is merely reduced to pieces; the succulence is expressed, and the fragments are formed into one mass.

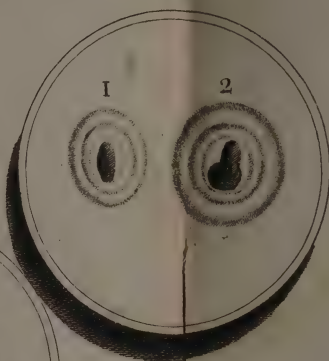
E R R A T A.

P. 8. l. penult. and elsewhere, for *turkies* read *turkeys*—P. 31. l. 23. for *one only* read *only one*—P. 48. l. 24. for *abounding* read *abound*—P. 62. l. 18. for *neck* read *beak*—P. 66. l. 6. for *canem* read *carnem*—P. 70. l. 16. between *them* and *being* insert *from*—P. 78. note, for 42 read 41—P. 83. l. 21. between *beat* and *when* insert *as*—P. 90. l. 6. for *are* read *is*—P. 97. l. 13. for *insinuating* read *insinuate*—P. 109. l. 11. for *rooks* read *ravens*—P. 110. l. 21. for *came* read *come*—P. 118. l. 15. between *wished* and *very* insert *it*—Ditto l. 16. for *progreſs* read *proceſs*—P. 124. l. 4. for *in* read *into*—P. 127. l. 32. for *ejected* read *rejected*—P. 128. l. 48. for *barbels* read *barbel*—P. 135. l. 21. for *or* read *nor*—P. 140. l. 15. for *axilla* read *axilla*—P. 143. l. penult. for *they* read *it*—P. 152. l. 5. for *where* read *when*—Ditto l. 24. for *crow* read *owl*—P. 155. l. 33. for *owl* read *screech-owl*—P. 163. l. 15. after *for* insert *as*—P. 166. l. 26. for *in* read *to*—P. 171. l. 32. for *bebeld* read *behold*—P. 172. l. 10. dele *one*—P. 173 l. 19. after *only* insert *would*—P. 184. l. 24. for *were* read *was*—P. 201. l. 16. before *appeared* insert *as*—P. 207. l. 4. after *examine* insert *them*—P. 215. l. 11. for *are* read *is*—P. 219. l. 21. for *analogus* read *analogous*—P. 231. l. 33. dele *if*—P. 274. l. 7. for *Gardana* read *Gardane*—P. 290. l. 14. after *in* insert *not*—P. 292. l. 30. for *choſe* read *chooſe*—P. 293. l. 25. dele *of*—P. 206. l. 21. for *miasma* read *magma*—P. 311. l. 27. the words *in the part that has been omitted* should have been at the bottom of the page—P. 316. l. 2. dele *blood*.

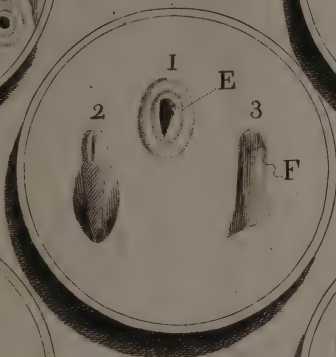
Fig. I



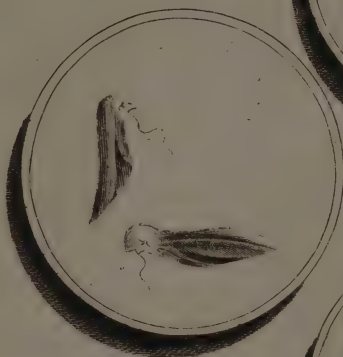
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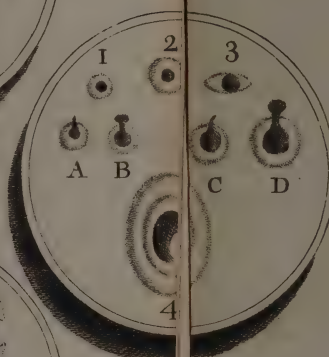
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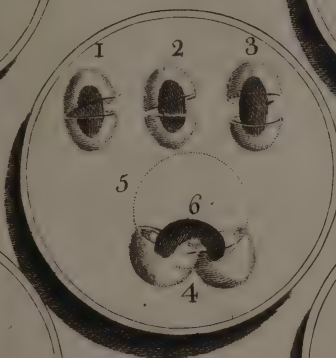
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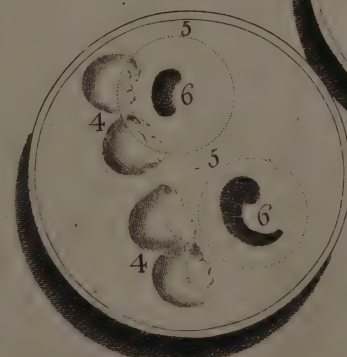
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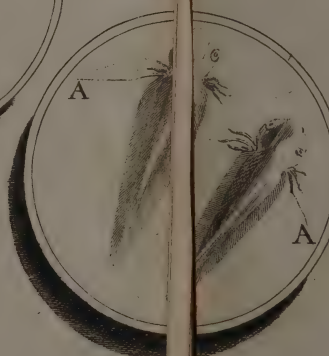
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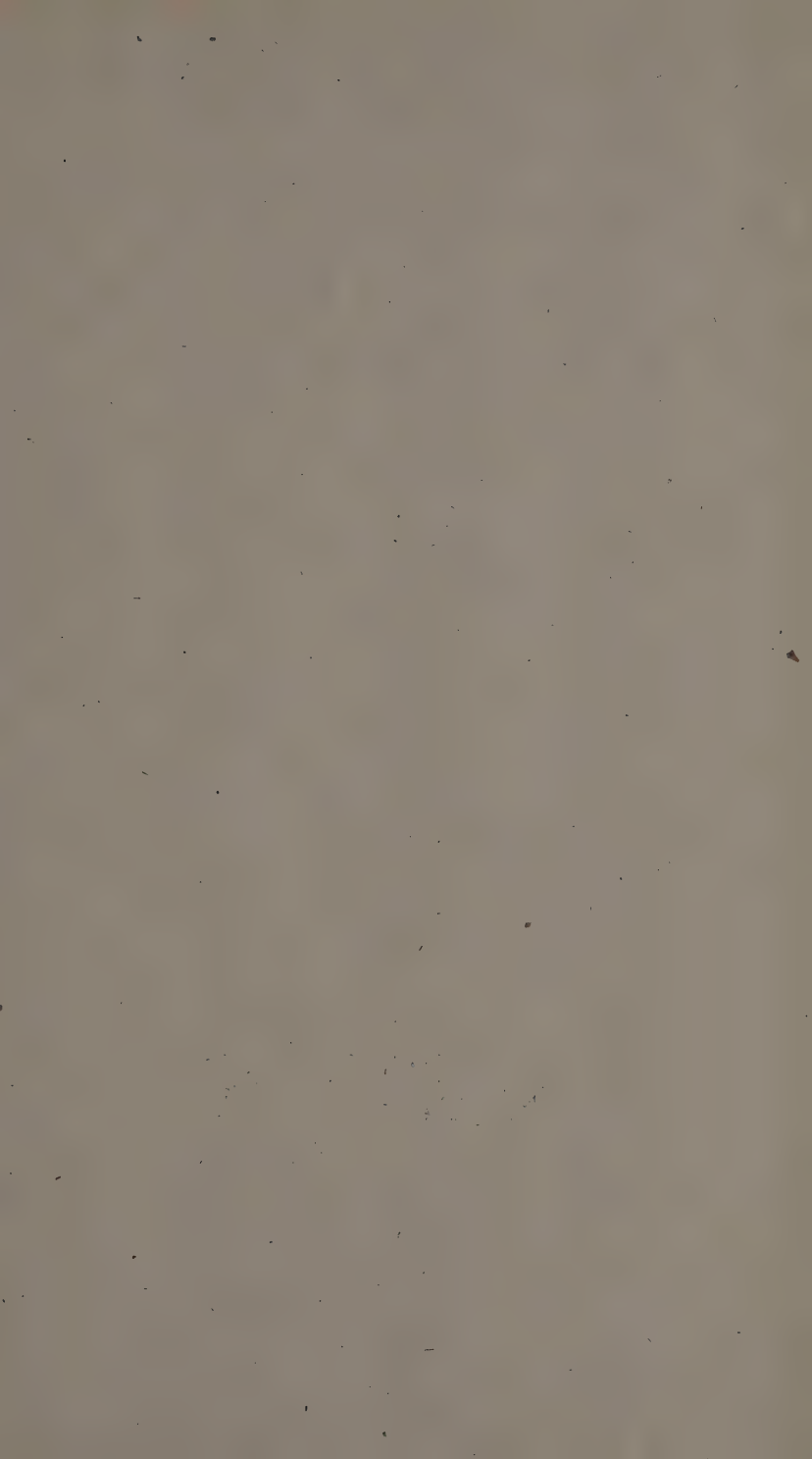


VII



VIII







Nat. Hist.
H

